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ART. I.—*Some Account of the Prangos Hay Plant of Northern India; prepared by permission of the Honourable Court of Directors of the East India Company. By Mr. John Lindley, F.L.S., &c. &c., Assistant Secretary at the Garden of the Horticultural Society of London.*

[Communicated by the Author.]

IN the north of India, in the neighbourhood of Imbal or Draz, grows a plant called Prangos; much employed as fodder for cattle, and of properties represented by the natives to be so marvellous, as to have excited doubts among the Europeans, whom reports of it had reached, as to its being more than an Oriental exaggeration. Owing to the little intercourse which takes place with the unfrequented districts where it was stated to grow, no opportunity occurred of gaining accurate information respecting it till the year 1822, when William Moorcroft, Esq., the superintendent of the Honourable East India Company's stud, on deputation to Upper Asia, having occasion to enter into communication with the Chinese authorities of Ela, undertook a journey to Draz, for the purpose of examining into the truth of the properties ascribed to the plant by the natives.

The information, thus acquired, appeared to this gentleman of such importance as to be worthy of an especial communication to the government at Fort William. Two chests of the seed, and

specimens of the Prangos Hay itself, were forwarded from India to this country, and presented by the Honourable Court of Directors to the Horticultural Society, with the correspondence between Mr. Moorcroft and the Indian government. Having had the honour to receive permission to use these important documents for the purpose of publication, I have prepared the following account of this remarkable plant, which may possibly become an object of great importance to our colonies in an agricultural point of view, whether we consider its amazing produce, its beneficial effects as a food for cattle, or the little care which is requisite in its cultivation.

The following are extracts from Mr. Moorcroft's letter, dated from Wakha, left bank of the Molbee Ches, 15th August, 1822:—

“ The plant called Prangos is employed in the form of hay, as a winter fodder for sheep and goats, and frequently for neat cattle; but its seed, when eaten by horses, is said to produce inflammation of the eyes and temporary blindness. The properties of Prangos as a food appear to be heating, producing fatness in a space of time singularly short, and also to be destructive to the *Fasciola Hepatica* or Liver Fluke, which, in Britain, after a wet autumn, destroys some thousands of sheep by the rot, a disease that, to the best of my knowledge, has in its advanced stages hitherto proved incurable. The last-mentioned property of itself, if it be retained by the plant in Britain, and there appears no reason for suspecting that it will be lost, would render it especially valuable to our country. But this, taken along with its highly nutritious qualities, its vast yield, its easy culture, its great duration of life, its capability of flourishing on lands of the most inferior quality, and wholly unadapted to tillage, impart to it a general character of probable utility unrivalled in the history of agricultural productions. When once in possession of the ground, and for which the preparation is easy, it requires no subsequent ploughing, weeding, manuring, or other operation, save that of cutting, and of converting the foliage into hay. Of its duration I have two facts, *viz.*, one of its seeds having been carried westward along with those of Yellow Lucerne, above forty years ago,

and sown on the eastern frontier of Kashmeer, where they vegetated, and where the plants of the *first* growth still remain in a flourishing condition; in the second instance, the seeds were transported eastward, and sown upon rocks near Molbee, where their plants flourished for about forty years, but in consequence of a long period of drought, during which there fell scarcely either rain or snow, the Prangos perished along with the crops of that district in general.

“ From various facts, it is conceived not unreasonable to presume that by the cultivation of this plant, moors and wastes, hitherto uncultivated, and a charge of disgrace to British agriculture, may be caused to produce large quantities of winter fodder, and that the yield of highlands and of downs, enjoying a considerable depth of soil, may be trebled. I have made every precautionary arrangement in my power by presents, &c., for gathering, drying, packing, and transporting a large quantity of the seed, and have left Mr. Guthrie, the apothecary, to superintend the operations. One cask will be transmitted through Kashmeer, and two others through Bushehar. And I take the liberty of submitting to the Most Noble the Governor-General in Council, the probability of this plant being of use to the new settlers, our countrymen, at the Cape of Good Hope, and to the colonists in general. As the Prangos has hitherto been of spontaneous growth alone, practices better adapted to the nature of the plant or of the country may be adopted at a future time; but from a view of its habitudes in its wild state, I venture to suggest that the seeds be dibbled singly into holes an inch deep and a foot apart, a short time before the rainy season.

“ During three years the plants will be little productive, but in that interim they will not be in the way of any other surface crop.”

Judging from the specimens sent by Mr. Moorcroft, each plant will produce about one and a half pound of dry fodder, which, allowing each plant to occupy four feet of ground when in perfection, will give a produce for bad land of more than a ton and three quarters each acre, which is nearly equal to the produce in

hay of the best English meadows. But if the distance recommended by Mr. Moorcroft be sufficient for the growth of the plants, that is to say, one foot, then allowing a plant to produce only half a pound of hay, an acre would yield the amazing weight of something more than nine tons and an half, a quantity which certainly appears to exceed credibility.

It is much to be regretted that from the length of time which elapsed between the despatch of the seeds by Mr. Moorcroft and their arrival in England, that is to say, from the 15th of August 1822, to the month of April, 1824, their vegetative powers had become so much exhausted as to render it extremely doubtful whether success will attend the experiments upon growing them. Now, however, that attention is called to the plant, other and speedier means may be employed for despatching the seed; no difficulty in procuring which can now be anticipated, Mr. Moorcroft having made arrangements with Ripghias, the Kenphun, and Mahomed Khan, the Chummul of Draz, for a supply of any required quantity of the seed.

The Prangos Hay Plant is a perennial herbaceous plant, with a large fleshy root-stock, usually measuring at the top from 18 to 22 inches in circumference, and formed by the aggregation of an infinite number of crowns or winter buds clustered together at or above the surface of the ground. The *crowns* are closely covered over by the coarse fibrous remains of the old leaves, by which the buds must be effectually protected from frost or accidents when the plant is in a state of rest. From each crown rises an abundance of finely cut leaves about two feet long, when dried, of a highly fragrant smell, extremely similar to that of very good new clover hay. They are supra-decompound, quite smooth, with linear, entire, or three-parted segments; their principal petiole is slightly sheathing at the base with a crisp thin margin; upwards it is solid, round, or slightly angular, with a smooth finely-striated skin. Of the secondary petioles there are from six to ten opposite pairs, according to the vigour of the leaf; they are in all respects like the primary petiole, except being smaller and more compressed, and having the first pair of their segments proceeding

from their very base. In these leaves the whole crop may be said to consist.

From the centre of the leaves rises the *flower-stem*, which I have only seen in a young and mutilated state. Good specimens of the *inflorescence* have not reached me; but from some imperfect umbels of flowers, I can state that the male and female flowers are produced upon distinct umbels. Of the *male flowers* the *umbels* are compound, shorter than the bractæ by which they are subtended, and both axillary and terminal; the *bractæ* are finely and deeply pinnatifid with three-parted segments, of which the end-lobe is broader than the rest, and often three-toothed. The *involucres* are both general and partial, each consisting of five or six membranous ovate-acuminate leaflets, which are shorter than the stalks of the umbellules, or of the florets. At the base of the umbel are clustered several scarious rudiments of florets. The *calyx* consists of five distinct ovate minute sepals. The *petals* are five, lanceolate, spreading, incurved, with a minute dorsal nerve. The *stamens* are five, spreading, the same length as the petals, and inserted opposite the sepals beneath a large, fleshy, slightly wavy *discus*, which surrounds two little processes, the rudiments of as many styles. The *filaments* are incurved, and quite smooth. The *anthers* large, square, innate, bilocular; each cell opening longitudinally with two valves. The *female flowers* have not yet been observed. The *fruit* is inferior, and consists of two united achenia, at maturity separating from base to summit from their common axis; it is oval-lanceolate, compressed, eight or nine lines long, and is crowned with two recurved styles, arising from the centre of a large, fleshy, wavy discus, and with the corky sepals of the persistent calyx. Of these achenia, the commissure or point of union is nearly flat, and narrower than their transverse diameter. Of each the pericarpium is corky, with five primary juga or elevations, which are in the centre produced into a corky wavy wing, and on each side covered densely with coarse tubercles; there are no secondary juga, and the valliculæ, or intervals, are concave and smooth. The *seed* is of the same form as the pericarpium, from which it is easily separable; it is covered

over with an indefinite number of colourless vittæ, both on the commissure and back; it has an involute horny albumen, and a minute, inverted, white embryo at its upper extremity; the cotyledons are flat and oval, the radicle rounded, and as long as the cotyledons.

From the foregoing description, which has been formed from such materials as have reached this country, it appears that the Prangos Hay Plant belongs to the natural order of Umbelliferæ, and that it bears much affinity to the genus *Cachrys*, with which it agrees in the corky nature of its pericarpium, in the absence of secondary juga, in having no vittæ, and in the involute structure of its albumen. With *Kruberia* of Hoffmann, which it resembles in the general appearance of its fruit, it may also be compared, notwithstanding its difference of habit; with that genus, however, it cannot be united, on account of its involute not solid albumen, numerous vittæ, and lanceolate not emarginate petals. From *Laserpitium* it differs materially in having involute albumen, an indefinite number of vittæ, and no secondary juga, while its primary juga, which in *Laserpitium* are obsolete, are in Prangos the most conspicuous part of the fruit. To *Rumia* of Hoffmann it is not referable because of its solid pericarpium, distinct winged juga, and long flat achenia.

To revert, therefore, to *Cachrys*, with which, as I have already stated, Prangos has many points in common: if *Cachrys Morisoni*, the fruit of which has a solid, corky smooth pericarp, with its juga nearly obsolete, is to be considered the species in which the essential character of the genus is to be sought, it is obvious that Prangos cannot be considered of the same genus. But if *Cachrys* be admitted in the form under which it has been placed by Sprengel in the sixth volume of Römer and Schultes's *Species Plantarum*, it is equally certain that the subject of this article cannot be separated from it. Differences in the fruit and petals of Umbelliferæ are, however, by the common consent of botanists, admitted to be of such importance in fixing the characters of the genera of that order, that a combination of plants, like that which has been attempted in the work above quoted,

must be considered utterly subversive of analytical division, and can only lead to a return to the genera of Umbelliferæ as Linnæus left them.

Besides *Kruberia* and *Rumia*, the distinctions between which and *Prangos* I have already explained, there is a third genus included in *Cachrys* by Sprengel, but separated by Professor Link, under the Bauhin's name of *Hippomarathrum*. From this *Prangos* seems principally to differ, in having entire, not pinnatifid, involucre, the juga winged, not rounded, and the petals lanceolate, not round with a broad involute segment; all points of great importance in characterizing umbelliferous plants.

Having thus shewn that the *Prangos Hay Plant* is strictly referable to no genus of Umbelliferæ at present constituted, I propose here to establish it with the following name and character:—

PRANGOS.

Char. Nat.—Calyx quinque-dentatus. Petala æqualia, lanceolata, incurva, integerrima. Discus carnosus, crispus. Achenia a dorso compressa. Pericarpium suberosum: commissura plana, angusta; jugis quinque primariis alatis, secundariis nullis. Semen multivittatum. Albumen involutum.—*Herbæ Asiæ temperatæ. Involucre universalia et partialia simplicia, polyphylla. Flores abortu monoici, lutei? Folia supradecomposita.*

Among the plants placed by Sprengel under his genus *Cachrys*, is the *Laserpitium ferulaceum* of Linnæus, found in the Crimea, a climate not very different from that of Draz, and described by Marschall von Bieberstein under the name of *Cachrys alata*. This having a winged corky fruit, like that of *Prangos*, and otherwise agreeing with it in character, the genus now established will consist of two species, which may be distinguished thus:—

1. *Prangos pabularia*. Mihi supra.

P. foliis glabris.

2. *Prangos ferulacea*.

P. foliis hirtis.

SYN.—*Cachrys orientalis ferulæ folio. Tourn. it. 2. p. 286 c. ic.*

Laserpitium ferulaceum. Linn. Sp. Pl. 358.

Cachrys alata. Bieb. Taur. Cauc. I. 217.

ART. II.—*Some Observations on the Physiology of Speech, read to a Literary Society, at Nottingham, on January the 3rd, 1825; by Marshall Hall, M.D., F.R.S.E., &c. &c.*

[Communicated by the Author.]

THE different parts which compose the mouth and nostrils constitute the organ of *Speech*, as distinguished from the *Voice*, which is formed in the larynx, or upper part of the windpipe; and speech may be defined as consisting in those modifications of the vocal sounds, produced by impressions made upon the air, *after* it has left the organ of voice. These impressions are chiefly effected in the mouth; the office of the nostrils is far more simple, being merely to admit the egress of the air in the articulation of certain letters, whilst it is intercepted in that of others.

The parts of the mouth which principally conspire to perform the functions of articulation, are the lips, the teeth, the tongue, the palate, and the *velum pendulum palati*, or pendulous vail of the palate. The offices of each of these parts will be explained as we proceed, and, with the exception of that of the part last mentioned, will be readily understood by every one. Of this last part, it is only necessary to say, that it forms a sort of curtained arch, hanging over the posterior part of the tongue, and may be readily seen, on looking into the throat. The function of the pendulous vail of the palate is, in the enunciation of certain letters, to close the nostrils at their posterior orifices; for this purpose, it is drawn upwards, and accurately applied to the orifices into the nostrils, by means of an appropriate set of muscles.

In the course of the subsequent remarks, we shall have abundant occasion to notice the wonderful manner in which the different parts of the mouth concur to perform their very varied functions, of which articulation is only one. It may not be irrelevant, indeed, very shortly to notice some other of the functions of these parts, before I proceed to the more immediate object of this essay. We shall thus be still more struck with admiration, that functions so very different should be performed

by the very same parts ; and we shall thus be better enabled to deduce the functions of some of these parts, or to confirm the conclusions we may draw, relative to the nature of these functions, as obtaining in the mechanism of speech.

I would then, in the first place, refer to the function of deglutition, or swallowing.—In order to effect deglutition, every cavity or canal with which the mouth communicates must be accurately closed, with the exception of the pharynx and gullet, along which the substance swallowed is propelled, whilst the various muscles by which deglutition is effected are called into action :—the mouth is thus closed in front by the lips, the windpipe is closed and accurately secured by a little valve-like body, called the epiglottis ; and the posterior openings of the nostrils are closed by the pendulous vail of the palate being drawn upwards and accurately applied to them. Thus is the cavity of the mouth completely shut, and excluded from its communications with the open air, or with the lungs. And if, from laughing or inadvertency, or from a defective action of the parts, as in palsy, any one of these functions be not adequately performed, the consequences are well known ; the substances which ought to have passed down the pharynx and œsophagus, escape out of the mouth ; or into the windpipe, inducing the most distressing coughing ; or in some instances, though more rarely, into the nostrils.

I notice, in the second place, a function of the mouth equally different from deglutition and articulation. It is performed in the act of sucking, and in that of blowing the blow-pipe. The first of these acts implies a diminished, the second an increased, elasticity and pressure, with regard to the air contained in the mouth ; and yet, during the continuance of these acts, we are enabled to breathe freely and uninterruptedly through the nostrils, whilst the cavity of the mouth is complete, and its posterior aperture closed by means of the pendulous vail of the palate, the windpipe and nostrils communicate freely behind that vail.

In the act of deglutition, then, the nostrils and the windpipe are accurately closed by their respective valves. In the acts just

described, on the contrary, the mouth is closed by the pendulous vail of the palate, now performing the office of a valve in a different situation, whilst the windpipe and nostrils communicate and remain open. We thus see the pendulous vail of the palate endued with a double office, the first being accurately to close the nostrils; the second, to close what has been termed the isthmus faucium. In the articulation of certain letters, again, we shall have occasion to observe, that the posterior orifices of the nostrils are closed, whilst the orifice into the windpipe is, of course, left freely open;—so wonderfully and variously are the functions of these delicate organs capable of being combined.

I now proceed to the more immediate object of this essay, namely, to explain the physiology or mechanism of articulation. I shall, first, notice the effect of speech on the respiration, or rather, on the act of *expiration*, which, it will be observed, is in some cases completely interrupted and arrested; in others, performed through the nostrils; in others, again, through the mouth alone. I shall, in the second place, describe the offices of the different parts comprising the organs of speech, in the articulation of the principal *consonants*; and in the third place, I shall trace the action and function of these parts in the enunciation of certain *vowels*.

We shall cease to be surprised at the fatigue expressed by persons whose office it is to speak much in public, when we have duly and fully examined the nature of the function of articulation. It may be ascertained, by the merest experiment, that in the pronunciation of the short word *BAT*, we adopt a mechanism, by which, not only are the different letters formed, but the respiration is twice completely arrested;—and that, in the pronunciation of the equally short word *FAN*, we first interrupt the flow of the air through the nostrils, whilst it is forced between the teeth and upper lip, and then intercept the course of the air through the mouth, and allow it to pass only through the nostrils.

Speech might be considered, indeed, first, as an exertion and trial of the muscular power; and secondly, as exerting a baneful influence, in certain cases, on the lungs themselves. The

trial of the muscular power, during speech, is known from the experience of public speakers, but is only fully appreciated by the physician, in his attendance on those cases of disease which particularly impair the muscular strength, as continued fevers; the degree of energy of the speech may, indeed, be considered an accurate measure of the degree of muscular power or debility, and the observant physician may learn from this alone, that his patient is getting worse, remains stationary, or is becoming convalescent. It is not less observable, that speaking has a baneful influence on the patient who labours under disease of the lungs; and it is said of the celebrated Talma, that he never performs *Les fureurs d'Oreste* without being taken with spitting of blood.

It is on their influence on the respiration, that I have formed my division and arrangement of the consonants; their sub-division may be founded on their respective modes, or mechanism of their enunciation. I shall, therefore, divide them—

1. Into those, in the articulation of which both the mouth and the nostrils are closed, and the respiration, of course, completely arrested:

2. Into those, in the enunciation of which the nostrils are closed, but the mouth left more or less open, for the exit of the air, which is compressed, but not interrupted, in its expiration:

3. Into those, not requiring even the nostrils to be closed, and in the enunciation of which the air is still less compressed in its course from the lungs: and,

4. Into those, in the articulation of which the expired air is not interrupted, and scarcely impeded at all.

Of the *first* class, are

B	T	C
P	D	K

In tracing these letters into their sub-divisions, we may observe, that the first pair are labials, being formed by the lips compressed together; the second pair are linguo-dentals, formed by pressing the point of the tongue against the posterior and upper part of the upper teeth; and the third pair are linguo-palatials, being effected by pressing the middle part of the tongue against the

palate. In all, the posterior apertures of the nostrils are effectually closed by the pendulous vail of the palate being drawn upwards, and accurately applied to their posterior apertures. And of course, those persons whose palate is perforated, or in whom the pendulous vail of the palate is imperfect, as sometimes arises from disease, are more or less incapacitated from pronouncing these letters, the expired air being no longer intercepted, as it ought to be, in its course.

Of the *second* class, are

F, hard
 the and TH, and S.
V soft Z.

In the articulation of these letters, the posterior orifices of the nostrils are required to be closed, whilst, in the first pair, the compressed air is continually forced between the teeth and upper lip; in the second, between the teeth and the tongue; and in the third, between the point of the tongue and the anterior part of the palate.

From this view of the subject, it will be readily apprehended, how the substitution of D or T for the TH, by foreigners, is so remarkable; for it is no less than the substitution of a total interruption, for a mere compression of the air in its exit from the chest.

Of the *third* class of letters, are

M, N, L, R.

In the enunciation of these letters, the expired air is only very slightly compressed, the nostrils being left freely open. It is for this very reason, probably, that these letters have been termed *liquids*, as flowing without obstacle. And it is by this circumstance, principally, extraordinary as it may appear, that the letter M differs from the letters B and P, for they are all equally labial; and that the letter N differs from T and D, for they are all equally formed by placing the point of the tongue near the roots of the upper teeth.

Of the *fourth* and last class, are

H, the Greek X, W, and Y.

In the enunciation of these consonants, the air appears to be scarcely compressed or impeded in its exit at all. This fact may, I think, account for the circumstance, that it has even been doubted, whether the two last letters be really consonants or no; and for the remarkable fact, that they cannot, as *consonants*, form the termination of any word.

These letters, preceded, as they are in this arrangement, by the liquids, lead us almost insensibly to the class of letters to be next noticed, namely, the *vowels*.

These are so called, from having been supposed to relate to the *voice* alone*. This, however, is obviously an error. The different parts forming the mouth, or organ of *speech*, are not less necessary to the enunciation of the vowels than to that of the consonants, or their function less appreciable, on carefully making the experiment. Thus, the French U is entirely labial; the letter E is dental; O, palatial; whilst the diphthong AW, and the vowels marked in the French language by the circumflex (Λ) are guttural.

The mechanism of the vowels is not, indeed, so obvious as that of the consonants. It is, however, sufficiently so to afford an illustration and explanation of several remarkable facts.

First, it may be observed, that any peculiarity of *articulation* in a language, imparts a peculiar expression or physiognomy to those who speak it. This is particularly observable in the enunciation of the French U, of which we have already spoken, which would not be the case if this letter were merely *vocal*.

Secondly, this fact is so certain, that a child may be taught to form such a letter at once, if his attention be taken from the book and directed to the countenance of the teacher, when it would be quite a task to effect this object in any other manner.

It may be observed, that when any given *vowel* sound exists in a foreign language, and not in our own, it is learnt with far greater difficulty than a new but distinct consonant would probably be; and the same observation would appear to apply to

* Blumenbach.

those consonants which are pronounced with but little compression of the expired air.

I thought it not improbable, at one time, that the pendulous vail of the palate was in part propelled against the posterior orifices of the nostrils, by the force of the expired air, in the act of pronouncing those letters which require the respiration to be arrested for their articulation. But this opinion is altogether disproved by the experiment of articulating these letters during *inspiration*. The course of the air is not less interrupted in this case than in the natural mode of speaking; so that it is plain, that it is by the action of its muscles alone, that the pendulous vail of the palate performs its functions.

The facts stated in the preceding part of this essay have been greatly confirmed by observing the effects of a perforation of the palate, which I have just had the opportunity of doing. The perforation was about equi-distant from the teeth and from the vail of the palate. The following phenomena were observed:—The patient could not swallow, or smoke, or whistle; on attempting to pronounce the letters B, D, S, V, &c., the action of the muscles of articulation was extremely imperfect, and attended by a hissing noise occasioned by the escape of the air through the perforation in the palate. The letters G and K could, however, be pronounced perfectly, the tongue being brought, in the enunciation of these letters, into contact with the palate, at a part posterior to the situation of the perforation.

Having made these observations on the physiology or *mechanism* of speech, I shall conclude this essay by several remarks, which may, probably, lead to some practical utility in the *education* of the faculty of speech, if I may be allowed that expression.

1. *Infancy* is, for two reasons, the period of life at which alone it is possible to teach the pronunciation of a foreign language perfectly; for it is at this age that the functions of the muscular system generally are fully developed, and that those of any particular part of this system are most readily confirmed into an easy habit. This fact is illustrated in the acquisition of execution in music, which is attempted in vain, even during a long

series of years, by any person after a certain age. The same observation applies to the pronunciation of a foreign language. It must be acquired early, or it will never be acquired at all. The French emigrants, who left their native country rather late in life, never acquired the pronunciation of our language, even so as to be readily understood. It is in infancy too, that the faculty of *imitation*, by which the mechanism of articulation is, in fact, principally, if not entirely, caught, exists in its greatest degree of excellency.

2. It has been observed, in regard to *stammerers*, or those who have a defective utterance, that they can *sing*, or even *read*, without hesitation, although they cannot speak. What is the *rationale* of this fact? I think it will be found to depend on the following principle. *Continuous* muscular action is far more easily effected than that which is interrupted. This principle is even general in physiology. It has been remarked, that a drunken man, or a person affected with that disorder termed St. Vitus's Dance, can *run*, though he cannot walk, or stand still*. In the same manner, a stammerer can sing, which is continuous motion, although he cannot speak, which is interrupted.

Continued muscular motion is also attended with less *fatigue* than that which is interrupted; and this is particularly observed in regard to speech. It is on this account, that there is a tendency in those who speak much in public to acquire a sort of continued sing-song mode of delivery, which it requires good taste and constant exertion to correct. It is on this account, too, that those who cry in the streets, actually acquire a sort of tune, or *cry*, as it is termed; the continued action of the muscles of speech being so much more easy than the interrupted. The same is constantly observed in children, on their first attempts to read.

Let a stammerer, then, observe this rule:—Always to speak in a continued or *flowing* manner, avoiding carefully all positive interruption in his speech; and if he cannot effect his purpose in this manner, let him even half sing what he says, until he shall,

* Heberdeni, Com. p. 93.

by long habit and effort, have overcome his impediment; then let him *gradually*, as he may be able, resume the more usual mode of speaking, by interrupted enunciation. I am persuaded, that this is the principal means employed by those gentlemen who have undertaken to correct impediments in the speech, and it is, undoubtedly, the most rational. In addition to this rule, let the stammerer endeavour to speak in as calm and soft a tone as possible; for in this way the muscles of speech will be called least forcibly into action, and that action will be least liable to those violent checks or interruptions, in which stammering appears to consist. It would, of course, be irrelevant to the object of this essay, to allude to those other principles connected with stammering, such as nervousness, of which it would be necessary to treat, in an essay written expressly on this interesting subject.

ART. III. *On some Cases of the Formation of Ammonia, and on the Means of Testing the Presence of Minute Portions of Nitrogen in certain states.* By M. Faraday, F.R.S., Corr. Member Royal Acad. Paris, &c. &c.

[Communicated by the Author.]

THE importance of the question relative to the simple or compound nature of any of the substances considered as elementary in the present state of chemical science, is such as to make any experimental information respecting it acceptable, however imperfect it may be. An opinion of this kind has induced me to draw up the following account of experiments relative to the formation of ammonia, by the action of substances apparently including no nitrogen. The experiments are not offered as satisfactory, even to myself, of the production of ammonia without nitrogen; indeed, I am inclined to believe the results all depend upon the difficulty of excluding that element perfectly, and the extreme delicacy of the test of its presence afforded by the formation of ammonia: yet as, on the contrary, notwithstanding my utmost exertions, I have failed to convince myself that ammonia could not be formed, except nitro-

gen were present, it has been supposed that the information obtained, though incomplete, might be interesting.

Having occasion, sometime since, to examine an organic substance with reference to any nitrogen it might contain, I was struck with the difference in the results obtained, when heated alone in a tube, or when heated with hydrate of potassa: in the former case no ammonia was produced; in the latter, abundance. Supposing that the potash acted, by inducing the combination of the nitrogen in the substance with hydrogen, more readily than when no potash was present, and would, therefore, be useful as a delicate test of the presence of nitrogen in bodies, I was induced to examine its accuracy by heating it with substances containing no nitrogen, as lignine, sugar, &c.; and was surprised to find that ammonia was still a result of the experiment. This led to trials with different vegetable substances, such as the proximate principles, acids, salts, &c., all of which yielded ammonia in greater or smaller quantity; and, ultimately, it was found, that even several metals when treated in the same way gave similar results; a circumstance which appeared considerably to simplify the experiment.

The experiment may be made in its simplest form in the following manner: put a small piece of clean zinc foil into a glass tube closed at one end, and about one-fourth of an inch in diameter; drop a piece of potash into the tube over the zinc; introduce a slip of turmeric paper slightly moistened at the extremity with pure water, retaining it in the tube in such a position that the wetted portion may be about two inches from the potash; then holding the tube in an inclined position, apply the flame of a spirit lamp, so as to melt the potash that it may run down upon the zinc, and heat the two whilst in contact, taking care not to cause such ebullition as to drive up the potash; in a second or two, the turmeric paper will be reddened at the moistened extremity, provided that part of the tube has not been heated. On removing the turmeric paper and laying the reddened portion upon the hot part of the tube, the original yellow tint will be restored: from which it may be concluded that ammonia has been formed; a result confirmed by other modes of examination to be hereafter mentioned.

The first source of nitrogen which suggested itself was the atmosphere: the experiment was therefore repeated, very carefully, in hydrogen gas, but the same results were obtained.

The next opinion entertained was, that the potash might have been touched accidentally by animal or other substances, which had adhered to it in sufficient quantity to produce the ammonia: the alkali was therefore heated red hot, as a preparatory step, and afterwards allowed to touch nothing but clean glass or metals; but still the same effects were produced. The zinc used was selected from a compact piece of foil, was well rubbed with tow dipped in alkali, washed in alkaline solution, afterwards boiled repeatedly in distilled water, and dried, not by wiping, but in a hot atmosphere; and yet the same products were obtained.

All these precautions, with regard to impurity from fingering, were found to be essentially requisite, in consequence of the delicacy of the means afforded by heat and turmeric paper for testing the presence of ammonia, or rather, of matter containing its elements. As a proof of this, it may be mentioned, that some sea sand was heated red hot for half an hour in a crucible, and then poured out on to a copper-plate, and left to cool; when cold, a portion of it (about 12 grains) was put into a clean glass tube; another equal portion was put into the palm of the hand, and looked at for a few moments, being moved about by a finger, and then introduced by platina foil into another tube, care being taken to transfer no animal substance but what had adhered to the grains of sand: the first tube when heated yielded no signs of ammonia to turmeric paper, the second a very decided portion.

As a precaution, with regard to adhering dirt, the tubes used in precise experiments were not cleaned with a cloth, or tow, but were made from new tube, the tube being previously heated red hot, and air then drawn through it; and no zinc or potash was used in these experiments, except such as had been previously tried by having portions heated in a tube to ascertain whether when alone they gave ammonia.

It was then thought probable that the alkali might contain a minute quantity of some nitrous compound, or of a cyanide, intro-

duced during its preparation. A carbonate of potash was therefore prepared from pure tartar, rendered caustic by lime calcined immediately preceding its use, the caustic solution separated by decantation from the carbonate of lime, not allowed to touch a filter or any thing else animal or vegetable, and boiled down in clean flasks; but the potash thus obtained, though it yielded no appearance of ammonia when heated alone, always gave it when heated with zinc.

The water used in these experiments was distilled, and in cases where it was thought necessary was distilled a second, and even a third time. The experiments of Sir Humphry Davy * shew how tenaciously small portions of nitrogen are held by water, and that, in certain circumstances, the nitrogen may produce ammonia. I am not satisfied that I have been able to avoid this source of error.

At last, to avoid every possible source of impurity in the potash, a portion of that alkali was prepared from potassium; and as the experiment made with it includes all the precautions taken to exclude nitrogen, I will describe it rather minutely, as illustrative of the way in which the other numerous experiments were made. A piece of new glass tube, about half an inch in diameter, was first wiped clean, and then heated red hot, a current of air passing at the same time through it; about six inches in length was drawn off at the blow-pipe lamp, and sealed at one extremity. Some distilled water was put into a new glass retort, and heated by a lamp; when about one-half had distilled over, the beak of the retort was introduced into the tube before-mentioned, and a small portion of water (about fifty grains) condensed into it. A solid compact piece of potassium was then chosen out, and having been wiped with a linen cloth, was laid on a clean glass plate, the exterior to a considerable depth removed by a sharp lancet, and portions taken from the interior by metallic forceps, and dropped successively into the tube containing the water before-mentioned. Of

* Phil. Trans. 1807. p. 11.

course the water was decomposed, and the tube filled with hydrogen; and when a sufficient quantity of solution of potash had been thus formed, the tube was heated in a lamp, and drawn out to a capillary opening, about two inches from the closed extremity. (fig. 1. plate.) The tube now formed almost a close vessel; and being heated, as the water became vapour, it passed off at the minute aperture, and ultimately a portion of pure fused hydrate of potassa remained in the bottom of the tube. The aperture of the tube was now closed, and the whole set aside to cool.

A piece of new glass tube was selected about 0.3 of an inch in diameter; it was heated to dull redness, and air passed through it: about ten inches of it was then cut off, and being softened near to one end by heat, it was drawn out at that part until of small diameter: (*a*, fig. 2. plate.) that part was then fixed into a cap, by which it could afterwards be attached to a receiver containing hydrogen. The tube containing the potassium potash being now broken in an agate mortar, a piece or two of the potash was introduced by metallic forceps into the tube at the open end, so as to pass on to the contracted part; a roll of zinc foil, about one grain in weight, cleaned with all the precautions already described, was afterwards introduced, and then more of the potash. The tube was then bent near the middle to a right angle; a slip of turmeric paper introduced, so as just to pass the bend, and thus prepared, it was ready to be filled with hydrogen.

The precautions taken with regard to the purity of the hydrogen, were as follows: a quantity of water had been put into a close copper boiler, and boiled for some hours, after which it had been left all night in the boiler to cool. A pneumatic trough was filled with this water just before it was required for use. The hydrogen was prepared from clean zinc, which being put into a gas bottle, the latter was filled entirely with the boiled water, and then sulphuric acid being poured in through the water, the gas was collected, the excess of liquid being allowed to boil over. The hydrogen was received in the usual manner into jars filled with the water of the trough, the transferring jar, when filled, being entirely im-

mersed in the water, so as to exclude the air from every part, even of the stop-cock. The first jar of gas was thrown away, and only the latter portions used.

The gas being ready, the experimental tube was attached to the transferring jar by a connecting piece, so that the part of it containing the zinc and potash was horizontal, whilst the other portion descended directly downwards. A cup of clean mercury, the metal being about an inch in depth, was then held under the open end of the tube, and by lowering the jar containing the hydrogen in the water of the pneumatic trough, so as to give sufficient pressure, and opening the stop-cock, the hydrogen in the jar was made to pass through the tube, and sweep all the common air before it. When from 100 to 150 cubic inches, or from 200 to 300 times the contents of the tube, had passed through, the cup of mercury was raised as high as it could be, so as to prevent the passage of any more gas, the pressure from the jar in the water-trough was partly removed, and the stop-cock closed; then, by lowering the cup of mercury a little, the surface of the metal in it was made lower than that within the tube, and in this state of things the flame of a spirit lamp applied to the contracted part of the tube, (*a*, fig. 2.) sealed it hermetically, without the introduction of any air, and separated the apparatus from the jar on the water-trough.

In this way every precaution was taken that I could devise for the exclusion of nitrogen; yet, when a lamp was applied to the potash and zinc, the alkali no sooner melted down and mingled with the metal, than ammonia was developed; which rendered the turmeric paper brown, the original yellow re-appearing by the application of heat to the part.

Still anxious to obtain a potash which should be unexceptionably free from any source of nitrogen, I heated a portion of potash with zinc, endeavouring to exhaust any thing it might contain which could give rise to the formation of ammonia: it was then dissolved in pure water, allowed to settle, the clear portion poured off and evaporated in a flask by boiling; but the potash thus prepared gave ammonia, when heated with zinc, in hydrogen gas.

With regard to the evidence of the nature of the substance produced, it was concluded to be ammonia in the experiments made in hydrogen, from its changing the colour of turmeric paper to reddish brown; from the disappearance of the reddish brown tint and reproduction of yellow colour by heat; from its solubility in water, as evinced by the greater depth of colour on moist turmeric paper than on dry; from its odour; and from its yielding white fumes with the vapour of muriatic acid. When formed in open tubes, its nature was still further tested by its neutralizing acids and restoring the blue colour of reddened bitumous paper; by its rendering a minute drop of sulphate of copper on a slip of white paper deep blue; and also, at the suggestion of Dr. Paris, by introducing into it a slip of paper moistened in a mixed solution of nitrate of silver and arsenious acid, the yellow tint of arsenite of silver being immediately produced.

These experiments upon the production of ammonia from substances apparently containing no nitrogen, will call to mind that made by Mr. Woodhouse, of Philadelphia, on the action of water on a calcined mixture of charcoal and potash, during which much ammonia was produced*; and also to the strict investigation of that experiment made by the President of the Royal Society during his inquiries into the nature of elementary bodies†. Sir Humphry Davy found that when one part of potash and four of charcoal were ignited in close vessels cooled out of contact of the atmosphere, pure water admitted to the mixture, and the whole distilled, small quantities of ammonia were produced. That when the operation was repeated upon the same mixture ignited a second time, the proportion diminished; in a third operation it was sensible; in a fourth barely perceptible. The same mixture, however, by the addition of a new quantity of potash, again gained the power of producing ammonia in two or three successive operations; and when any mixture had ceased to give ammonia, the power was not restored by cooling it in contact with air.

Sir Humphry Davy refrains from drawing conclusions from

* Nicholson's *Journal*, xxi. 290. † *Phil. Trans.* 1809, p. 100, 1810, p. 43.

these processes, observing with regard to the composition of nitrogen in these experiments, that till the weight of the substances concerned and produced in these operations are compared, no correct decision on the question can be made: I am anxious to be understood as imitating the caution of one whose judgment stands so high in chemical science; and, therefore, draw no positive conclusion from the experiment I have described, or from the results I have yet to mention. As, however, I think they may lead to elucidations of the question, I shall venture to give them, not with the minute detail of the preceding experiment, but in a more general manner.

Potash is not the only substance which produces this effect with the metals and vegetable substances. Soda produces it; so, also, does lime, and baryta, the latter not being so effective as the former, or producing the phenomena so generally. The common metallic oxides, as those of manganese, copper, tin, lead, &c., do not act in this manner.

Water or its elements appear to be necessary to the experiment. Potash or soda in the state of hydrates generally contain the water necessary. Potash dried as much as could be by heat, produced little or no ammonia with zinc; but re-dissolved in pure water and evaporated, more water being left in it than before, it was found to produce it as usual. Pure caustic lime, with very dry linen, produced scarcely a trace of ammonia, whilst the same portion of linen with hydrate of lime yielded it readily.

The metals when with the potash appear to act by, or according to, their power of absorbing oxygen. Potassium, iron, zinc, tin, lead, and arsenic evolve much ammonia, whilst spongy platina, silver, gold, &c., produce no effect of the kind. A small portion of fine clean iron wire dropped into potash melted at the bottom of a tube, caused the evolution of some ammonia, but it soon ceased, and the wire blackened upon its surface; the introduction of a second portion of clean wire caused a second evolution of ammonia. Clean copper wire, in fused potash, caused a very slight evolution of ammonia, and became tarnished.

The following, among other vegetable substances supposed to

contain no nitrogen, have been tried with potash in tubes open to the air; lignine, prepared by boiling linen in weak solution of potash, then in water, afterwards in weak acid, and finally in water again; oxalate of potassa, oxalate of lime, tartrate of lead, acetate of lime, asphaltum, gave very striking quantities to tumeric and litmus paper: acetate of potash, acetate of lead, tartrate of potash, benzoate of potash, oxalate of lead, sugar, wax, olive oil, naphthaline, produced ammonia, but in smaller quantity: resin appeared to yield none, nor when potash was heated in the vapour of alcohol or ether, or in olefant gas, could any ammonia be detected.

It may be remarked, that much appeared to depend upon the quantity of potash used; sugar, for instance, which with a little potash would with difficulty yield traces of ammonia, does so very readily when the quantity of potash is doubled or trebled; and linen, which with potash gives ammonia very readily, yields it the more readily, and in greater quantity, as the proportion of potash is increased.

The experiments with the substances which contain carbon, assimilate, in consequence of the presence of that body, with the one by Mr. Woodhouse. Whether the substances act exactly as charcoal does, probably, cannot be decided until the correct nature of the action is ascertained; but there are apparently some very evident differences. The ammonia, in the charcoal experiment, does not exist until after the ignition, nor before the addition of water; but in several experiments of the nature of those described in this paper, the ammonia is evolved before the substances acting or acted upon, are charred. Thus, if linen fibre, cut small, be mixed in the tube with hydrate of lime, and heated, ammonia is evolved before the heat has risen so high as to render the linen more than slightly brown; and oxalate of potash, in a tube with potash, when heated, gives much ammonia before any blackening is produced.

Mr. Woodhouse's experiment may be very readily repeated, though not in an exact way, by heating a little tartrate of lead with potash, in a tube in the flame of a spirit lamp, driving off the

water and first products, and raising the residue to dull redness. If a drop of water be allowed to flow down on to the residue when cold, and it be then heated, ammonia will be found to rise with the water.

I was induced in the course of these experiments to try again and again, whether the potash or lime would not yield ammonia when heated alone; but when well prepared, and the tubes experimented in perfectly clean, they gave no indications of it. By exposure to air for three days in a room, hydrate of lime appeared to have acquired the power of evolving a little ammonia when heated, and caustic lime so exposed gave still stronger traces of it. Potash also exhibited an effect of this kind, and potash which had been heated with zinc, and contained oxide of zinc, most decidedly. Some potash and zinc were heated together; a part was immediately put into a clean close bottle; another part was dissolved in pure water, decanted, the solution evaporated in a covered Wedgewood's basin, and then also set aside in a close vessel for 24 hours: at the end of that time the first portion, heated in a tube, gave no decided trace of ammonia, but the latter yielded very distinct evidence of its presence, having apparently absorbed the substance which was its source from the atmosphere during the operations it had been submitted to. White Cornish clay being heated red hot, and then exposed to the air for a week, gave plenty of ammonia when heated in a tube. When the substances were preserved in well-stoppered phials, these effects were not produced.

Such are the general and some of the particular facts which I have observed relative to this anomalous production of ammonia. I have refrained from all reasoning upon the probability of the compound nature of nitrogen; or upon what might be imagined to be its elements, not seeing sufficient to justify more than private opinion on that matter. I have endeavoured to make the principal experiments as unexceptionable as possible, by excluding every source of nitrogen, but I must confess I have not convinced myself I have succeeded. The results seem to me of such a nature as to deserve attention, and if it should hereafter be proved

that nitrogen had entered in some unperceived way into the experiments, they will still shew the extreme delicacy of heat, or heat and potash, as a test of its presence by the formation of ammonia.

With respect to the delicacy of the test, it may be observed that it offers many facilities to the detection of nitrogen when in certain states of combination, which chemists probably were not before aware of. A portion of asbestos, which had been heated red hot, was introduced into a tube by metallic forceps and heated, it gave no ammonia; another similar portion compressed together, and introduced by the fingers, gave ammonia when heated. A very minute particle of nitre was dropped into hydrate of potassa, and heated to dull redness, it gave no ammonia; a small piece of zinc foil, dropped in and the heat applied, caused an abundant evolution of that substance.

The circumstance also of absorption by lime and other bodies, of something from inhabited atmospheres, which yields ammonia when thus tested, is very interesting; and Dr. Paris has suggested to me that this power may probably be applicable to the examination of the atmosphere of infected and inhabited places, and may perhaps furnish the means of investigating such atmospheres upon correct principles.

February, 17, 1825.

ART. IV.—*Description of the Coal recently discovered on the Estates of the Count de Regla, in the Intendancy and Kingdom of Mexico. By Th. Stewart Trail, M.D., F.R.S. E., &c.*

[Communicated in a Letter to Mr. Swainson.]

THE mineral treasures now laid open to the skill and enterprise of British adventurers in South America, are daily exciting an increased interest throughout the kingdom. And as connected with the powerful machinery that will be employed in these un-

dertakings, the subject of FUEL becomes one of the greatest importance. The woods and forests, which once clothed the sides of the Cordilleras in the vicinity of the principal mines, have been, for many years, gradually diminishing, and in many places have totally disappeared; while the Mexican proprietors, with singular negligence, have forgotten to form new plantations to supply that enormous quantity of fuel necessary for the mines.

The existence of Coal on the mining provinces of Mexico has hitherto been very doubtful. Humboldt, indeed, mentions it has been found in New Mexico; and that the formations of basalt and amygdaloid on the estates of the Count de Regla might lead to the belief that this substance also would probably be discovered; a supposition likewise entertained by Mr. John Taylor, whose practical and scientific knowledge of mining is well-known. These opinions are now completely verified; as among the mineral productions brought by Mr. Bullock from Mexico, are specimens of a coal analogous to *jet*, which he procured while residing in the vicinity of Real del Monte. A small piece of this substance, weighing sixteen grains, has been analyzed by Dr. Trail, and the result of his experiments, contained in a letter to Mr. Swainson, is expressed in the following words:—

“ This specimen is more analogous to *jet* than to our Wigan Cannel coal. Its colour is deep brownish black; its lustre resinous; its cross fracture conchoidal; its longitudinal fracture has a slightly fibrous appearance, as if it had originally been wood. Its hardness is about that of Cannel coal, as is its frangibility; but its lustre is higher. The mean of three careful experiments gave its specific gravity = 1.2248. It becomes considerably electric by friction; in this character it is analogous to *jet*, and differs from Cannel coal, which scarcely shews any symptoms of electricity by friction. Though I have observed that some pieces of the latter slightly moved an insulated cat's hair, which is a very delicate electroscope. Kirwan considers the difference between *jet* and Cannel in their electric energies, as a diagnostic mark.

“ It burns with a lively flame, and gives out much liquid

bituminous matter, or *coal tar*, so as to cake or become semi-liquid in the fire. It does not decrepitate when heated, like Cannel. When heated before the blowpipe, in a glass tube, its volatile parts are separated; and it leaves behind about 50 per cent. of a coke which is capable of exciting a pretty strong heat. The volatile portion affords a *very pure* coal gas. Six grains of it, burnt in a platina crucible, left behind 0.2 grains of greyish white ash, which is equivalent to $2\frac{1}{3}$ per cent. of incombustible matter in it.

“ The smallness of the specimen rendered it impossible to ascertain the relative quantities of carbon, hydrogen, and nitrogen, which similar substances contain, but this sort of analysis is rather an object of curiosity than utility.”

V.—*On the Origin, Materials, Composition, and Analogies of Rocks*, by John Mac Culloch, M.D., F.R.S.E.

[Communicated by the author.]

IF it is the first error of the observer to see, like the miner, but a very limited number of rocks in the system of nature, it is not long before he falls into one, the very reverse; creating for himself permanent distinctions from every incidental variety that comes under his notice. Time, however, speedily corrects this error, and teaches him, that however the aspects of rocks may be multiplied, Nature has limited these productions by a very confined set of general and constant characters.

Of the Constituents of Rocks.

A small number only of the earths which chemistry has discovered, forms the materials of all the rocks; united, in some cases, with alkalies, and with certain metallic oxides. In some, a single earth is found; in others, two or more exist; and these are either mechanically mixed, or united by the laws of chemical affinity. Thus are formed those rocks which are considered simple; simplicity, as applied to rocks, meaning simplicity of

aspect. Limestone presents an example of a rock in every respect simple ; while basalts and clay slates, although simple as rocks, are chemical compounds and mechanical mixtures.

Besides these distinctions, the earths are sometimes formed into separate minute bodies, or minerals, which are again united, so as to constitute rocks ; and these may be in themselves either simple or compound minerals. Sandstone offers an example of a simple rock of this kind ; simple in its chemical nature, but an aggregate as to its general character. Hornblende rock is an example of an analogous aggregate, but one in which the integrant minerals are chemical compounds. But there are differences here, even in the mode of aggregation : which, in some cases, results from the chemical interference of a simultaneous crystallization ; in others, from the mere mechanical aggregation of the parts ; and lastly, from the union of those two processes. Granular limestone is an example of the first, and instances of the last are to be found in different varieties of sandstone.

In compound rocks, different kinds of minerals are visibly united into a common mass ; which thus presents a sort of uniformity throughout the whole, however the separate parts may differ. Such compounds may consist of two or more minerals ; and, within certain limits, they seem to be ruled by laws as general as the simpler rocks. These compounded rocks vary, like the former, in being purely crystalline or otherwise ; and as granite presents a familiar example of the first, so quartz rock, and some of the compound argillaceous schists, afford instances of the other two.

There is still another description of compound rocks, to which the term conglomerate has been applied. In these, not only different minerals are united in a mechanical, a mixed, or a chemical manner, but fragments of former rocks, either simple or compound, also enter into their structure. Such fragments vary in size, from the most minute visible particles, to others of many pounds, or even hundreds of pounds weight ; and these rocks offer, in consequence, numerous varieties, which are fully treated of in the author's *Geological Classification of Rocks* ;

the repository for the facts of this description which do not appertain to the present paper.

The earths, which enter into the composition of those minerals that form the ordinary or essential ingredients of rocks, are silica, alumina, lime, and magnesia. If the other earths are occasionally found, it is rather in those minerals which cannot be considered essential to the constitution of rocks, but which are frequently imbedded in them. To these earths must be added, iron in different states of oxidation; and, from some observations which I have made on quartz rock, limestones, and traps, in that of a carbonate also. Potash and soda are, lastly, essential ingredients in some rocks, and it remains to be proved whether lithia may not sometimes be present where one or other of these has been suspected. As the earths, as well as the alkalies, are now known to be oxides, and as it is also known that silica, at least, acts the part of an acid in some mineral combinations, it is probable that we have much yet to learn, respecting the origin and formation of many rocks; but whatever splendid probabilities may open on us from this new source of knowledge, we are scarcely yet able to build any rational conjectures on it.

The simple minerals formed of these substances, and which constitute the essential ingredients of all rocks, are quartz, felspar, mica, hornblende, hypersthene, diallage, augit, serpentine, compact felspar, actinolite, chlorite, talc, and schorl. Some of these are, however, far more abundant than others; nor is it easy to define the limit between them and those which may be considered accidental; which are occasionally imbedded in rocks, as their natural repositories. But it is unnecessary to dilate on a subject which is sufficiently detailed in the work above-mentioned. It is sufficient to quote, as examples, garnet, which is sometimes abundant in micaceous schist, or sparingly dispersed, or altogether absent, without affecting its essential characters; and spodumene or corundum, which may thus exist in granite.

If we consider the great number of minerals, thus generally distinguished into essential and unessential, which nature has formed, or if even we limit our views to those which may be

considered as most essential, it is interesting to observe how few are the rocks which are produced from them. If the varieties are most numerous in the primary or older series, they are still few, and, within certain limits of variation, very constant. In the later rocks, the varieties are still more limited; and, when we reflect on the circumstances under which they have been produced, they are confined to a much less number than could have been expected. As most of the minerals of ordinary occurrence are formed, for example, of the earths which exist in granite and gneiss, why might we not expect to find garnet, corundum, or andalusite, in every one; instead of being, as they are, limited, to a few occasional specimens. We are equally at a loss to account for those distinctions between gneiss, micaceous schist, quartz rock, or other substances, that occur in the same ancient series; distinctions which, on the great scale, are really steady and definite, notwithstanding the occasional interferences of character that occur in particular cases. That these have been regulated by certain chemical laws, is unquestionable, however incomprehensible the nature of these may be. It must also be from this cause, that such rocks are found to preserve the same characters, wherever they occur; a circumstance otherwise calculated to excite our surprise. In every other department of nature, her productions vary according to the climate and situation, but granite is the same in Egypt and in Greenland. It is with the laws of organization alone that climate interferes.

As the secondary, or later, strata have been chiefly formed from the waste of the ancient rocks, it is less surprising that they should preserve a general constancy of character throughout the globe, however individuals may vary in different places. Even these variations are still remarkable; as well from their steadiness, as from the extent through which that uniformity can sometimes be traced. The difference between compact limestone and chalk, is no less remarkable than the similarity which, in distant places, occurs between strata that we can scarcely conceive to have formed parts of one deposit. It is worthy of remark, however, that, in the secondary strata, the most conspicuous

variations occur in the limestones; and these, it is obvious, have been subject, in many instances, to chemical laws, as well as to the influence of organized bodies, from which the others have been comparatively exempted. That the secondary strata should contain sandstone and schists, is easily accounted for, by recollecting that these must be the result of the destruction of the older rocks; the more durable mineral remaining distinct, while the compound ones have been reduced to powder. But the question of the formation and origin of all these rocks will be examined more particularly hereafter.

On the Consolidation of Rocks.

As almost all the rocks with which we are acquainted have been formed out of our sight, the mode in which the earths, or simple minerals, became consolidated into these forms, is to us a matter of inference from analogy, not of observation. If discussion could have determined this question, it would have been solved long since; as most of the schemes which have been called Theories of the Earth have been chiefly engaged in this pursuit, and as neither argument nor assumption has been spared in attempting to establish the exclusive views of many of these theorists. To record the terms under which the different partisans have thought fit to array themselves, would be to foster and perpetuate an opposition, often arising, more, perhaps, from the colours of the different banners, than from the merits of the cause. Blue and green factions have often exerted their influence beyond the limits of eastern or western empires, and in far other pursuits than politics.

Fortunately, all rocks have not been formed in the depths of the earth, and fortunately, also, it is in the power of art to produce some of these substances from indiscriminate mixtures of their elements. It is our business to try how far we can extend analogies from the visible to the invisible, from the present to the past. If this process will not carry us far, it is at least the only rational mode of investigation in our power.

Volcanoes are among the most active and impressive sources

of those rocks which are now daily forming on the surface of the globe. By the agency of their fires, the earths are ejected in a state which, as far as we know, is merely that of mixture, and united in the fluidity of fusion. By repose, during a process of slow cooling, various combinations take place in these fluid masses; and, according to circumstances which we are but imperfectly able to appreciate, there are formed numerous rocks, either apparently simple, or compounded of the different minerals that have been formed by the contending affinities of the materials. These processes are imitable by art; which, having first reduced the natural compounds furnished in basalt or other rocks, to a fluid and uniform glass, in the laboratory fires, disposes them so as to cool during long repose, in a gradual manner. Thus, by the slow cooling of the most compounded materials of the glass-house furnace, various imitations of rocks are formed; and thus, more precisely, the greenstones of the trap family are destroyed, and again regenerated.

In examining now those rocks which have been formed out of our sight, we find one family which produces many counterparts to the volcanic rocks, namely, the family of trap. So absolute, indeed, is the identity between many members in each set, that no eye nor any analysis can distinguish them. To attempt to prove this by an enumeration of specimens in each, would be only to give a list of names that would carry no conviction. But no more convincing proof is wanted than this; that, to this moment, geologists continue to dispute about what belongs to the trap family, and what is of volcanic origin; not only in countries remote from volcanoes, or no longer containing the marks of former activity; not only in the Vivarais and the Euganean hills, but at the very seats of living volcanoes. If, therefore, out of a common mass of rock, or among many different ones evidently formed under the same circumstances, there are parts which bear all the marks of an origin similar to that of volcanic rocks, it is evident that the whole must be referred to the same source, with certain exceptions arising from collateral circumstances that it is not within the limits of this paper to notice.

Thus analogy, resemblance, and experiment, confirm that opinion respecting the trap rocks, which would be inferred from the peculiarities of their chemical constitution; and thus also they confirm the conclusions that may be drawn from their peculiar disposition, and from the nature of their connexion with the various conterminous rocks among which they are found.

It is but a step from the trap rocks to granite; and if the identity of specimens is not always so perfect, or the resemblance so general and extensive between these and the volcanic rocks, the analogical reasoning is quite as unexceptionable. I showed, in a former paper in this Journal, that many rocks, forming integrant portions of a granite mass, are undistinguishable from many of the trap rocks, and that among these there are many that resemble the productions of volcanoes. Here, then, is an identity, even between granites and volcanic rocks; and here, also, what is true respecting the origin of one part of the mass, must be true respecting the whole. If that inference appears to be drawn closer than the circumstances seem to warrant, we may carry it through the intermediate stage of trap; and having thus proved the identity of this rock with the volcanic products on the one hand, and with the granite on the other, apply a common mathematical axiom to the conclusion.

If it be said that volcanoes do not produce granite, it must still be recollected that they produce compounds of an analogous nature in every respect. It was also shewn, in the paper referred to, that the trap rocks often assumed the character of perfect granite; so that, by this intermediate step, the several products which are most distant are again associated. Even admitting that the volcanic rocks stood exclusively at one extremity of a scale of chemical compounds, and the granites at the other, the trap rocks containing examples of both, form the common link by which they are united. This view of the chemical origin of granite is confirmed by the same set of appearances which confirm it in the case of the trap family, and which have been sufficiently described by various writers.

It is not difficult to assign probable reasons for the differ-

ences in the chemical appearances of the rocks in these three distinct productions. They have, however, been sufficiently pointed out, on other occasions; and it has been shewn, that they probably consisted, in a great measure, in differences of the time through which the fused materials had cooled, circumstances confirmed by a great number of collateral appearances, although, in many cases, there can be no doubt that great differences have resulted from the different proportions of the several earths in the fused compounds.

Thus, from chemical analogies, there is assigned to all the unstratified rocks, that origin which was already deduced from various other considerations; and thus there is proved to exist a division of rocks formed exclusively by the agency of heat. It will now be convenient to begin the remainder of this examination at the other extreme.

Where water holding carbonate of lime in solution is gradually evaporated, there are formed calcareous concretions, which often attain a great size through age, and which, under peculiar circumstances of crystallization, are sometimes not very different in aspect from certain limestone rocks. Under different circumstances, similar waters deposit their contents, so as to form rocks of great depth and extent, producing real calcareous strata. The Travertino of Italy appears to be one of the most perfect examples of this nature. These simple and recent calcareous rocks become compounds, in cases where the calcareous solution has entangled fragments of shells, as it does in the West Indian islands at this day, or where it has united fragments of discordant natures, as it does on the shores of Messina, and on many of our sea coasts. Thus calcareous rocks, both simple and compound, are formed by water. Lastly, rocks of this nature are now daily produced, in many parts of the great ocean, by the efforts of marine animals; the deserted coralline structure being cemented partly by the actions of the animals themselves, and partly by that of the sea on the calcareous earth. In the same manner, ancient submarine piers, as at Carthage, become cemented through lapse of time, by the intervention of shell-fish

and the solution of their calcareous matter, into masses of solid rock. In this way, calcareous rocks are formed, partly by chemical agency, and partly by that of submarine animals.

Where iron becomes converted from the metallic or oxidulous state to that of rust, it becomes the cement of all the smaller materials within its reach; and thus sandstone is often formed on sea-shores, in sand and gravel beds, and, very probably, to a considerable extent in the noted ferruginous sand stratum of England.

Thus two modes of producing rocks, by the agency of water, are demonstrated. It remains to inquire, what probability there is, that the same agency can convert silica to that end, as we cannot produce any instances so perfect, of its absolute action in that way.

The solubility of silica in water cannot be a matter of dispute, however difficult it may be to effect its solution in our laboratories. In my work on the Western Islands, I have produced nearly all the instances of this nature that are required for the present purpose; but I may here add to these, its actual solution in the hot waters of Iceland and Italy, and the consequent production of siliceous tufas and stalactites. To convert this property to the present purpose, it is not requisite that the solution be very extensive, or very rapid. If we conceive this agent acting for a long series of years in a mass of loose sand or of clay, it is not difficult to see that the final result must be, in the first instance, the formation of a sandstone, and, in the other, probably, that of a schist.

That this is the fact, in nature, is almost demonstrable, from the frequent partial occurrence of sandstones in beds of loose sand, and from the mixed chemical and mechanical texture of almost all the solid sandstones. This effect, it is true, has sometimes been attributed to the action of heat; but to adduce as an agent that which cannot be shewn capable of producing a given effect, while we are in possession of one that has the desired power, is to abandon sound reasoning for the sake of maintaining a species of fictitious analogy, which, after all, is not ne-

cessary for the support of that theory by which it was so anxiously defended.

Thus there have been produced two distinct sets of causes for the formation of rocks; the first chiefly applicable to the unstratified substances, and the last to the formation or consolidation of strata.

Mr. Playfair has objected to the possibility of aqueous consolidation on these grounds; that a liquid solvent could not exclude itself from the pores of the rock after depositing the consolidating matter, and that it should, therefore, remain within the stone, or else leave the body pervious to water; "neither of which is" said to be "the fact." On the contrary, both of these propositions are true. The presence of water in stones is so universal, that I have never yet found any rock in which it did not exist, when that could be procured quickly from a sufficient depth. It is contained even in granite, and in the trap rocks; and the great change of colour and hardness which many of the latter undergo after being formed into specimens, is owing to its evaporation. Thus specimens of augit rock, which have the waxy soft look and green colour of serpentine, when fresh broken, become black in a few days. It is also known, that small granite veins are sometimes found perfectly soft in the quarry; and these harden in a few days, apparently by the evaporation of their water, and the consequent precipitation of silica, or else by the nearer approximation of their parts. In Sky, I have found masses of granular quartz or sandstone, which could be moulded by the hand when first found, but which, in the same manner, became solid in a few days. In all these cases, the loss of weight proves the presence of water, as it does the porosity of the stones. Even the common quartz of veins contains water, under the same circumstances; losing both weight and transparency on drying. The porosity of stones, as well as the presence of water, are thus both proved by the same facts. But the former property ought never to have admitted a doubt; since the compactness of flint and agate are apparently far greater than that of any rock, compound or simple, and since these not only give pas-

sage to oil, but even permit sulphuric acid to follow, and to precipitate the charcoal within the pores of the stone.

That the water in stones is actually saturated with earths, and probably with silica or lime, appears to be also proved, by certain appearances which take place on breaking and drying some of these. In marbles raised very wet from the quarry, a whitish dusty surface soon follows, from the deposition of the carbonate of lime; and it is probable, that a similar cause will account for that gray tarnish which is produced in pitchstones, within a very few hours after the specimens are broken from the rock; during which process of drying, they also become far more compact, or less tender. Thus the objection in question falls to the ground; were it even necessary that the process of consolidation should be reserved for that time at which the whole stratum was completed.

Of the different Rocks, and the Modes of their Consolidation.

Now although a large portion of the strata of the globe may have been formed by this last process of aqueous solution, and a considerable portion, at least, of the secondary ones probably owe their origin or consolidation to this cause, there are many strata, particularly in the primary or older series, to which it is impossible to apply it, so as to explain all the appearances which they present. It will here be convenient to point out in succession, those which may have been consolidated merely from water, ending with those which will not admit of that explanation; and it will remain to inquire, whether the phenomena cannot be explained by the successive agency of both the causes which have been examined. It will also be seen, that in one instance, at least, among these ancient strata, either cause separately might have produced the effects visible.

There is nothing in the character of quartz rock, as far as I have examined it, to prevent it from having been consolidated to its present condition from the long continued application of an aqueous solution of silica. But that it was deposited from water originally in the state of sand and gravel, is rendered evident

from the rounded and foreign fragments of discordant rocks which it often contains. At the same time, there is no reason to deny that it may have been exposed to the action of heat, as it is still capable of undergoing that without suffering any change. That it was consolidated by heat we cannot prove; and are scarcely in a condition to deny, that it may not have been partly indebted for its constitution to that cause.

If shale could be indurated from water alone, there would be no reason to deny that the same cause may have operated in the primary argillaceous schists; while, that they have been deposited from water, is proved by the fragments and the shells which they so often contain. Here again, however, we are in the same condition as with regard to quartz rock; unable to prove that it may not have experienced in some degree the action of heat; as we know, from observations on the siliceous schists, that shells are not necessarily obliterated in these circumstances. But that action, if it existed, cannot have been very great; as we are certain, both from experiments and observation, that it is either fused or indurated by this cause. The very existence of siliceous schist in the vicinity of trap and granite, produced by the action of these rocks on shale and slate, not only prove this fact, but shew the very limits where the action of heat ceases.

Thus, two important members of the primary strata are, probably, indurated from water alone. With respect to limestone, it is now known, both by direct experiment and by observation on the effect produced by trap veins on chalk, that it may be crystallized from fusion, provided the escape of the carbonic acid is restrained. It has been shewn, that it is equally consolidated from water; and, on examining this limestone in its various associations, its origin must probably, in some instances, be referred to one of these causes—in others, to the other. It is likely, for example, that all the limestones associated with clay slate are derived from watery deposition and crystallization; and it is probable that those associated with gneiss have received their present condition from heat. This opinion is justified by many circum-

stances; such as by their giving passage to granite veins, by the change of chemical texture and composition which they present in these cases, and by the crystallization within them of minerals similar to those found in gneiss, such as garnet, hornblende, augit, and others, which could not have been deposited from water so as to have entered into the confused crystalline arrangement of the rock in the manner which they do. That limestone is actually thus consolidated after fusion, even in large masses, is also proved, as far as anything relating to the influence of trap is proved, by the conversion of conchiferous secondary strata, in those situations, into crystalline limestone; a fact occurring very extensively and demonstrably in Sky, and recorded in my work on the Western Islands.

With respect to serpentine, the whole question is as yet involved in darkness. It is not known that it can be formed from water, and I have proved from observation, that, as it passes into trap, forming part of a greenstone vein in Perthshire, it can be formed by fusion.

All the scaly schists, of which micaceous schist may here represent the whole, present characters which are scarcely explicable without admitting the action of these two agents. The stratified disposition, and the laminar form, both give indications of a deposition from water; and, if any doubt of that could remain, it is removed by finding that, in many places, it contains fragments of discordant rocks,—of granite, for example, limestone, and quartz rock. It has further been held, that the parallel position of the mica is, in itself, a sufficient proof of deposition, because it is the necessary position, and because the same circumstance exists in the micaceous sandstones, so analogous to it, which are actually deposited from water. But this, if probable, is an equivocal circumstance: as I have shown, that in hypersthene rock, a member of the trap family, and even in some rare trap veins that contain mica in the Western Islands, the flat crystals of hypersthene in one case, and the mica in the other, preserve that parallelism which must here be attributed to the polarity of crystallization operating extensively; an action which I have also elsewhere

shown to have been sometimes exerted throughout the felspar of granite veins.

But admitting that micaceous schist was deposited, like the secondary micaceous sandstones, from water, and consolidated by the same means, it presents characters which cannot be explained by this process. If its flexibility has not been the consequence of heat, which I have elsewhere attempted to prove that it has, the peculiarities of its crystalline texture and occasional contents cannot be explained, without admitting that it has been exposed to a heat sufficiently intense and sufficiently durable, to permit these minerals to be formed in the same manner as they are in granite and in the volcanic rocks. The condition and existence of garnet, hornblende, tourmalin, staurotide, and other minerals, are inexplicable by any mode of watery deposition, and still less by any subsequent crystallization from water.

I need take no notice of diallage rock, or of the more ancient red sandstone; as the same processes of reasoning apply to them as to those rocks to which they are analogous; but hornblende schist requires a particular consideration. This is an extremely fusible compound, and its peculiar crystalline texture proves that it could not have been deposited from water; in which, indeed, its earths are insoluble, and from which they could not thus have been precipitated. It is, besides, precisely analogous to many greenstones of the trap family; from which, indeed, it is often so little distinguishable, that it has been confounded with them, by those who choose to believe in the aqueous origin of trap, under the name of primitive greenstone. That it is further actually produced by heat, is evinced by finding that the argillaceous schists, when in contact with granite, are actually converted into it. Whether simple, or compounded of hornblende and felspar, the same reasoning applies to it. It is, nevertheless, admitted, that its original materials have been deposited from water, and thus its laminar and stratified disposition is explained. That it has further consisted of clay or schist, is not only rendered probable by the numerous facts occurring in the trap rocks, but by

that very striking analogy, now at last so well understood, in which beds of shale beneath trap are actually converted into Lydian stone; a substance differing from it, almost solely in the compactness and uniformity of its texture.

We thus lastly arrive at gneiss; a rock which often bears the marks of igneous consolidation in a still greater degree than those of aqueous deposition, but in which it is almost unquestionable that both have been combined. Where gneiss is at a distance from granite, its laminar and stratified disposition is most perfect; where in its vicinity, it is most obscure; indeed, so obscure, as at length to disappear. This is precisely what might be expected to happen on this view of its double origin; namely, the application of heat in unequal degrees, to a series of beds deposited from water, and, probably, like quartz rock, originally consolidated from it also. Where it is most remote from granite, although its mineral materials should be the same, they are disposed in a different manner; or are more rigidly laminar and more independent. Where it is most immediately in the vicinity of that rock, and more particularly when it abounds in granite veins, the structure becomes analogous to that of granite, or to one in which there is that mutual penetration of crystals which can only take place in a fluid of fusion. At length it actually passes into the contiguous granite; losing that parallelism of the parts, and those last remains of the laminar disposition, which had gradually been decreasing.

It is by no means difficult to imagine this combination of causes and of effects; a state of softening or semifusion, sufficient to allow the integrant parts of a stratified watery deposit to enter into new combinations, and to crystallize without the loss of the original marks of stratification. These, indeed, are often preserved in gneiss, by the alternate interposition of beds and laminæ of hornblende, and by that only; just as in the watery joint deposit of sandstone and shale, the latter substance is often the only indication of stratification that can be procured.

That such re-crystallization can take place in a rock which is heated to a point short of actual fluidity, is proved by Mr. Watt's

experiments, so often quoted; and that strata can, in nature, lose all their indications of watery deposition, while they preserve the stratified shape under a new mineral form, is evinced by the existence of siliceous schists beneath trap, as already quoted. A greater degree of heat and a longer continuance of it, are all that are required to produce all the differences in these cases; and the fact, of the frequent interposition of hornblende schist between beds of gneiss, is strongly confirmatory of the consistency and truth of these views. Thus, also, the transition of gneiss into granite becomes a phenomenon of easy solution.

Of the General Causes of Consolidation.

I need not here terminate this view of the consolidation of these primary rocks, by any general inquiries respecting the origin of the heat or its diffusion. Nothing can be said on this subject that has not been often said; and whatever difficulties may occur in attempting to apply these principles rigidly to every case that may be examined, it can only be said that this theory offers a general and obvious solution of the facts; and that if it cannot be exactly fitted to meet every exigency, it is no more than must happen in every similar case of a general principle, when we are not in possession of all the collateral circumstances by which it may have been modified.

In thus deducing, both from the agency of heat and of watery solution, the consolidation of all the stratified rocks, and in limiting these according to the various circumstances that have been indicated, it must be apparent that the power granted to the former is comparatively small, and that it is not here supposed to have acted beyond the range of the more ancient rocks, probably not through the whole of these. It is very possible, nevertheless, that the action of heat may have been much more extensive. But that it has acted in the consolidation of the secondary strata at large, is rendered in the highest degree improbable, by a variety of circumstances which I need not enumerate, because they have frequently been urged against the whole theory, to which the name of a party has been given.

This is one of the unfortunate results that sometimes follows from attempting to prove too much; from overstraining an argument, so as to give advantages to the adversary, who, in finding a weak point, imagines that one blow will slay his enemy. It is not very good philosophy to disregard an obvious cause for the purpose of adopting a possible one; and it is a subject for regret, that those to whom geology is indebted for many rational views have too often exposed themselves to this censure.

But, in admitting that the great mass of secondary strata have been consolidated by a watery agent, it must be remembered that there is a wide difference between the consolidation and the precipitation of the same substances from water. If every one of these rocks did not give the most unquestionable proofs of its having originated, either in the ruins of more ancient rocks, or in the spoils of animals, it would be a sufficient argument against precipitation from a watery solution, that it involves every species of chemical and mechanical impossibility that can be included in a proposition so simple. It is unnecessary at present to detain the reader a moment longer on an hypothesis that would create and destroy oceans at its pleasure, yet find them ineffectual.

No notice has yet been taken of the power of mere pressure, either in actually consolidating rocks, or in assisting their consolidation. Yet it is an agent not to be overlooked; and when we consider the enormous weight to which the strata must have been subjected, it is very easy to conceive that its power cannot always have been inefficient. The occasional compression and fracture of imbedded shells, proves that it has sometimes acted; and if even the most delicate of these bodies are generally preserved, it only proves that they were well supported by the surrounding materials, not that they have not been subjected to great pressure. In our own experiments, with forces far inferior, clay can be compressed into a substance as hard as shale; and there are many of the schists not so hard as the heterogeneous mixture that is forced into a rocket, although composed of materials from which such an effect could scarcely be anticipated.

[This Paper will be concluded in our next Number.]

ART. VI.—*On Light and Heat from Terrestrial Sources.*

By Baden Powell, M.A., F.R.S.

[Communicated by the Author.]

To the EDITOR of the *QUARTERLY JOURNAL of SCIENCE*, &c.

DEAR SIR,

YOUR readers will be aware from the reports of the proceedings of the Royal Society, that, on the 17th of February last, a communication from me was read on the subject of radiant heat; having been for some time engaged in the inquiry into the nature of the effects produced by the radiation from *luminous hot bodies*, as distinguished from that emanating from *non-luminous* sources, I have made many other investigations besides those contained in the paper alluded to. Some of these, comprised in the following remarks, may be considered as supplementary to the primary inquiry made in that paper; and they lead to a very simple theory of a subject which has hitherto been much involved in confusion. Should you favour me by inserting them in your Journal, I trust they may not be altogether uninteresting to your readers; especially as some of them are connected with topics which have formed the subject of some of your own experiments.

I remain, dear Sir,

Yours, very truly,

BADEN POWELL.

My whole inquiry is grounded upon the following assumptions, which, I conceive, are warranted by the most decisive experiments of Leslie, De la Roche, &c.

1. That simple radiant heat from *non-luminous* sources, produces its effect on bodies exposed to its influence in proportion to a certain peculiarity of *texture* in their surfaces, which is the same as that which gives them a greater power of radiating heat, and is altogether independent of colour.

2. That simple radiant heat is incapable of permeating glass

(at least of ordinary thickness,) however transparent. Whatever doubts may have been entertained on this point, I think are completely removed by the investigations of Dr. Brewster, in his paper on "New Properties of Heat," &c.—*Phil. Trans.* 1816, Part I., Prop. 40, &c.

3. That from *luminous* hot bodies a very considerable heating effect is produced, essentially different from that just described. It *passes through glass* by direct radiation, without heating it, and affects bodies in proportion to the *darkness of their colour*, without relation to the texture of their surfaces. This is evident from the results of M. De la Roche, (*Biot. Traité de Physique*, Vol. iv., p. 640, &c.) Mr. Brande, (*Phil. Trans.* 1820, Part I.); and other experiments.

4. That of the total heating effect from this class of bodies a considerable portion is stopped by glass. This appears from De la Roche's experiments; and further it is shewn that the degree in which this interception takes place, decreases in proportion as the body becomes more intensely luminous.

The theory adopted by Professor Leslie, as also apparently by M. Biot, in his account of the Relations of Light and Heat, (*Traité de Physique*, Vol. iv., p. 640, &c.), is, that this interception takes effect upon one simple agent, which is heat, more or less converted into light, according to the stage of combustion, &c., and is greater, as the agent approaches more nearly to the form of simple heat, in which case it is entirely stopped, or nearly so. In order to establish this theory, it would be necessary to shew, that whatever may be the particular law of relation to surfaces, by which the action of the "igneous fluid" is determined at any stage of its evolution, the portion intercepted should bear the same relation in this respect as the portion transmitted. At the higher degrees of incandescence, for example, this relation is to the darkness of colour. The same relation ought consequently to hold good with the portion intercepted, as with that transmitted by the glass. At lower stages, an approach to the preference for absorptive *texture* would be displayed, and this again should be equally found in both cases.

The experiments which I tried, were of a nature calculated to give an answer to this question, whatever result they should present. If they shewed the same relation maintained under the two different conditions, the theory of *one agent* would be established. If a different relation appeared, we must, of necessity, infer *two distinct radiations*.

In order to proceed with the following remarks, I must, of necessity, assume the result of these former experiments as established; but as it would be improper here to detail the contents of that paper, I shall merely state the general conclusion, which is, indeed, already before the public.

This conclusion, in fact, simply determines the question just proposed, in favour of *two distinct radiant agents*, or species of heating effect, which act jointly in the emanation from luminous hot bodies: of these, one is simple radiant heat, resembling in all its properties that from non-luminous bodies; that is, *stopped by glass*, and having relation to the *texture*, not the *colour*, of surfaces on which it acts: the other, a sort of effect associated in the closest manner with the rays of light, *passing with them through glass*, and affecting bodies in proportion to their darkness of *colour*, without respect to the texture of their surfaces. This last, for the sake of distinction, I call, "the heating power of light."

2. I may here premise a short account of one description of experiments, which I at first employed, in resolving this question, and which is not given in the paper alluded to.

These experiments were conducted upon the following principle, which, though very simple in theory, is in practice attended by several inconveniences, which, if not carefully guarded against, may lead to error.

A differential thermometer, having one bulb coated with smooth black, and the other with absorptive white, was exposed to the radiation from luminous hot bodies, having both bulbs at an equal distance from the body, first with, and then without the interposition of a glass screen. The arrangement is represented in figs. 1 and 2 of the annexed sketch. (Plate II.)

If the screen had no heating or cooling effect, it is evident that

in whatever proportion the radiant matter from any source affects the two bulbs, if that radiant matter be of one simple kind, the only difference on removing the screen will be, that the *intensity* of its action on the bulbs will be *increased*; but it will act on each in the same proportion as before; consequently an increase of effect, or a motion in the same direction as before, must, of necessity, take place. If, therefore, we perceive the reverse of this take place, it is a decisive proof that when the screen is removed, a *different agent* is brought into action.

But the effect of the screen will probably interfere to too great a degree to allow of this conclusion, without further precautions.

The influence of the screen will be reduced to nothing, if we can operate at a sufficient distance. Or again, we may judge of its effect by moving it nearer to the source of heat, when its cooling influence (if any) must be diminished. In observing the effect produced on the instrument during a short interval of time, 30 seconds for example, the effect of the screen must be of a *cooling* nature; and since, on the principle above stated, equality of distance in the bulbs is not an essential condition, we may place them obliquely, (fig. 3.) care being only taken that they remain exactly in the same condition when the screen is removed; in this way the difficulty is obviated. The black bulb being the worst radiator, by placing it nearest the screen, the cooling power will be displayed in the less apparent effect on this bulb; but if, in this position, an effect take place on this bulb from the transmitted portion of the radiation, it will be evident that on removing the screen, the effect, if due to one simple agent, ought to increase, from a twofold cause, the removal of the cooling mass, and the admission of a new portion of heat to the bulb.

In various trials of this sort, I never found an increase under these circumstances, and often a decrease; that is, the action on the other, or more absorptive, bulb was now increased; or the portion of the heat before intercepted has a different relation to surfaces from that transmitted. The effect was observed from incandescent metal, and from the flame of lamps and candles.

In these experiments, the effect is often very small; I have

placed no reliance upon them alone ; and only mention them as additional variations, possessing considerable simplicity in their principle.

3. Having, as I conceive, in the investigation alluded to, established the distinct existence and joint operation of simple radiant heat, and the other heating agent, which for distinction I call the heating power of light, in the emanation from luminous hot bodies, it may not be uninteresting to examine the application of this doctrine to the results of former experimenters. In general it will be sufficiently obvious that the distinction thus established must apply to many well-known facts. Thus Mr. Brande in his paper on combustion, &c. (*Phil. Trans.* 1820, Part 1, Sect. 2,) states, that he obtained a considerable heating power on a blackened thermometer from the light of a flame of olefiant gas, collected by a lens, which did not become heated. This was evidently the effect of that portion of the radiant matter which is simply light possessing a power of communicating heat when absorbed : whilst all the other portion, namely the simple radiant heat was stopped by the glass ; and was tending, if the experiment had been continued, to heat it.

I need not proceed to state the application of the same mode of explanation to various other results, as it must immediately occur, but there are several particulars respecting the ratio obtaining between the two parts of the total effect radiated from different sources, which it appeared to me very desirable to examine, in reference to the phenomena attending the evolution of radiant matter.

Upon the principle laid down, we may view the important results of M. De La Roche * from these : if we estimate the light by the effect through the glass screen, and the heat, by the total effect minus this last, we may obtain the following values for the ratio $\frac{l}{h}$ in the different cases :

$$\text{Iron at } 427^{\circ} \text{ centig. } \frac{l}{h} = \frac{1}{7} \text{ nearly}$$

* *Biot. Traité de Physique*, Vol. IV. p. 640.

$$\text{Copper incandescent} = \frac{1}{2.4}$$

$$\text{2d exp.} = \frac{1}{2.8}$$

$$\text{Lamp, no chimney} = \frac{1}{2}$$

$$\text{—— with chimney} = \frac{1}{0.9}$$

In all the above instances, the value given to (*l*) should be slightly diminished for the heating effect of the screen. Also, the last ratio is of necessity inaccurate, because the simple heat was radiated from the glass chimney, and is probably greater than that which would be radiated directly from the flame. This ratio ought therefore to be increased. But even without this correction, it will hence be evident that in this series of luminous hot bodies, the heating power of light increases in a greater ratio than the simple radiant heat. And this increase of ratio corresponds to the degree of incandescence in the metal, and the completeness of combustion in the flame.

4. In order to prosecute this inquiry, I made use of the following application of the differential thermometer: if both the bulbs present vitreous surfaces, or are equally absorptive, the black bulb being affected by the light from a luminous hot body, and both bulbs being nearly equally affected by its simple heat, by interposing a small glass screen between the source of heat and the *plain* bulb, the effect of *both* is exhibited on the black bulb, or (*l+h*). Observing in the usual way we have the effect of the *light*, or (*l*). Hence we get (*h*) and the ratio of the two.

This arrangement is represented in Figs. 4 and 5. The influence of the two heating agents distinguished by the differently-dotted lines appears as in the former cases. Perhaps the numerical results obtained by such a mode of experimenting may not be susceptible of a great degree of precision, but for the purpose of ascertaining whether there was *an increase* of ratio or not, this

method is probably sufficient. In mentioning a few results which I have in this way obtained, I do not therefore conceive it necessary to go into numerical details.

5. It is well known that the general inferences made by De La Roche, with respect to the increasing *transmissibility* of heat in proportion to the degree of *luminosity*, have been confirmed by results of other experimenters on different principles, and in some instances the connexion between increase of *light*, and the peculiar *chemical* conditions of the case, have been established. By comparing such results, and viewing them on the principles now established, we get the connexion between those *chemical* conditions, and the increase in the *heating power of light*.

Thus with respect to an increased intensity of combustion, Count Rumford shewed (*Phil. Essays*, i, 304,) that in proportion to this increase the illuminating power increased. De La Roche has shewn that in the same proportion the transmissibility of heat is increased. Viewing this according to my principle, there is a corresponding increase in the ratio of the two radiations or $\frac{l}{h}$.

I have also confirmed this by applying the method above described (4) to the radiations from an Argand lamp, when its flame was in different stages of brightness; when a regular increase in the ratio was observable.

6. In the further investigation of this point, we may make some inferences from a consideration of Mr. Brande's experiments, (on gases, &c. *Phil. Trans.* 1820, Part 1.) He has there compared the heating effect by conduction of the flames of several species of gas; and has also estimated their relative illuminating effects. From the results given of the quantities of each gas requisite to produce equal lights and equal heats, we may deduce the proportions of the *effects* of heat and of light to be nearly as below, on the assumption that the heating is proportional to the illuminating power of light, and the radiant heat to the temperature in the different frames. Of the ratio in the same flame we can infer nothing.

GASES.	Ratios of	
	light	heat
Olefiant	5.	2.5
Oil	1.9	1.5
Coal	1.	1.

Hence it would be obvious, that the heating power of light increases in a greater ratio than the radiation of simple heat in this series of flames.

If we compare these results with the remarks of Sir H. Davy, that the increase of light depends on the increase of solid product, it will be evident, from the chemical nature of the flames employed in this comparison, that not only the increase of light, but also the increase of *ratio* between the light and heat, takes place, in accordance with Sir H. Davy's doctrine. (See *Phil. Trans.* 1817. Part i. p. 75.)

7. But these inferences are grounded entirely on the assumption, that the heat radiating from different flames, is in a proportion not greater than that of the elevation of their temperature. This, perhaps, may not be much questioned. But the other part of the assumption, *viz.*, that the heating is proportional to the illuminating power of light from different flames, may admit of doubt. Thus, it would become very desirable to extend the examination, on the principle here suggested, to the flames of different gases. This part of the inquiry I have not the means of attempting; but there are other phenomena of a description closely analogous to these, and which, coming within the reach of familiar examination, I have made the subject of experiment, so, as in some degree, to supply the deficiency.

8. One case regards the alteration which takes place in a flame, as exhibited in the simple experiment of placing salt in the wick of a spirit lamp: the effect being increased by, at the same time, diluting the spirits with water. (See *Dr. Brewster's paper. Edinb.*

Phil. Journ. No. xix. p. 123.) This experiment gave a corresponding increase of ratio when the density of the flame was thus increased.

Another very striking instance is found in the phenomenon observed by Count Rumford and Mr. Brande, that the quantity of light is increased by placing several flames near each other, or so as to coalesce. Count Rumford considered the cause of this to be that the flames thus communicated heat to each other, or, in other words, that a portion of the heat otherwise radiated away, was retained. This then, it would seem, is engaged in some way in the greater extrication of light. To this phenomenon I applied the same method of observation, and found the ratio increase as it should do, if we could assume that the heat radiated increases in proportion to the temperature of the flame. For Count Rumford found the light to increase in much greater proportion than the temperature of the flame.

I found, for example, that when two flames of wax candles were made nearly to coalesce, the heating power of the light was considerably more than double that from one of the flames, whilst the radiant heat was less than doubled.

A series of trials with incandescent iron, gave a decrease of ratio corresponding to the degree of cooling, till the mass ceased to be luminous.

9. In Mr. Brande's paper, before referred to, one of the most interesting and important results, is the fact, that the chemical power possessed by the solar rays is not found in any sort of terrestrial light, except that produced from intense galvanic action. This would seem to indicate that galvanic light forms a term in the series, approaching more nearly to the solar light, than the most intensely luminous flame; and, since the law of inverse proportionality between the two radiations continues through all the instances of combustion, and is again exhibited in the solar rays when the proportion of the radiation of simple heat has become insensible, it is most probable, that if the galvanic radiation were examined by the method above proposed, we should find, corresponding to its resemblance, in chemical power, an increase in the

ratio of the heating power of light, to the simple heat, considerably beyond what is found in the most intense flames. This experiment I have not had an opportunity of trying: it is to be hoped this notice may lead to its being tried. And, perhaps, the further investigation of this point may promote some advance towards a knowledge of the nature of the chemical influence accompanying light under these circumstances. A power, of which, at present, we can only say that it is exerted by the solar rays *where no radiation of simple heat* is present; and in the greatest degree by those rays which have the *least heating power*.

10. It forms one of the most interesting topics of inquiry to examine the nature of the solar heat. A variety of experiments have long convinced me, that this heat consists *solely* of that kind which belongs to light. Among other modes of trial, I have often applied that here employed: which though not of itself sufficient to *establish* the two radiations, will yet infallibly shew whether there be present the smallest radiation of *non-transmissible heat*. With an instrument graduated according to Professor Leslie's scale, a variation of the 20th of a centigrade degree may be distinguished. I have repeatedly tried the experiment with the screened bulb, both plain and coated with whitewash, or with white silk. The screen could here be placed at a sufficient distance to preclude all interference. And, in these cases, after waiting till the instrument placed in the sun had become perfectly stationary, I never perceived the slightest increase of effect on interposing the screen.

11. These experiments afford us a point of comparison between solar and terrestrial heat. The former resembles, in all its properties, one species of the latter. But this is always accompanied by another species totally distinct.

With respect to the nature of the former, or the heating power of light, various opinions have been held. And it is to an examination of these opinions, and of the conclusions which we may deduce on the subject, from the experiments here described, that the remaining portion of my remarks will be devoted.

With respect to the heating power of the solar light, we have not at present many data from which we can deduce a view of its

nature, otherwise than by analogy with the similar power possessed by terrestrial light. This we can trace to its source, and may thus be enabled to form opinions with tolerable certainty as to its nature.

12. Philosophers have been much divided in their opinions respecting the nature of the relation thus subsisting between light and heat. One party have maintained the absolute identity of those agents; accounting for the different properties exhibited by this heat, and by simple heat, only by supposing some modifications to take place in the state or form in which the "igneous fluid" exists, and by which it becomes either light or heat, according to circumstances; each possessing many of the properties of the other. Another opinion has been that of the totally distinct existence of the two; although they accompany each other in the closest and apparently most inseparable state of connexion.

The former of these opinions appears to me little more than a gratuitous assumption; and the latter is attended with insurmountable difficulties, if we suppose the heat so accompanying light to retain its separate existence and radiant properties. These, in fact, are entirely changed.

13. It appears from the most decisive experiments, that the sun's rays have a power of producing *heat* in bodies in proportion to the degree in which their surfaces (according to the common expression) *absorb light*, from their darkness of colour.

The cause to which this effect is to be attributed, whatever may be its nature, is clearly shewn to be so closely associated with the rays of light, that it seems impossible, under any ordinary circumstances at least, to effect a separation between them. It is transmitted with light through transparent bodies; increases or decreases with the intensity of light; and as nearly in exact proportion as the nature of the appropriate experiments will allow us to ascertain. Professor Leslie considers the proportion to be precise and undeviating: it is refracted with light to a focus; and communicates no heat to transparent media through which it passes; it is reflected with the rays of light, and polarized with them. It is also, as is well known, subjected to a peculiar distri-

bution among the primary rays when analyzed by a prism. But the conclusion once maintained of an absolute separation of heating rays by this means, has been more recently shewn to be most probably owing to red rays of so deep a colour as not to be ordinarily visible. (See *Mr. Herschel's Paper, Edinb. Trans.* 1823, §. 7.)

And though the recent experiments of Dr. Seebeck have shewn that this heat is distributed to different parts of the spectrum in very different degrees, according to the different nature of the prism employed, yet since the light is known to be also similarly affected, this is no proof of the distinct existence of separate rays of heat.

Upon the whole, then, we must view these effects as due to a certain power or property of communicating heat belonging to the rays of light. But this need by no means involve the supposition that light and heat are identical, or can be converted the one into the other. But with respect to this view of the subject, we may obtain greater certainty by returning to the subject of the foregoing experiments.

14. At the commencement of this inquiry I viewed it as bearing upon the theory, which asserts that the radiant matter from luminous sources is of one species, only gradually changed from heat into light. The facts here established so far disprove this opinion, that we evidently perceive a very considerable portion of the radiant matter undergoing no change whatever, except an increase in intensity.

If, therefore, we still adhere to the supposition that light is only heat in a different state, we must so far modify the hypothesis as to admit that only a part of the igneous fluid undergoes this change.

15. But without assuming any hypothesis, it is evident that the total effect, whether communicated directly, or by means of the light, must have originated in some way from the hot body; and although I have proved the total effect to be distinguishable into two parts or species, we still cannot consider them otherwise than as both derived from the same source. I conceive it already suf-

ficiently established that a portion of the heat of the radiating body actually disappears, (at least in the form of radiant heat,) or is in some way totally changed in its properties: yet it may be worth while to observe, that the experiments now recorded, or quoted, afford a more palpable confirmation of this conclusion.

16. When we consider that in every part of a flame an intense chemical action is going on, and a high temperature in consequence generated, it will follow that if by any external means the intensity of that action is increased, a proportional quantity of *heat* must be generated, and the intensity of such action is increased in the instance of a more complete combustion being produced by external means. That the quantity of light evolved, and perhaps also its intrinsic heating power, undergo an increase, whilst the simple heat does not increase so fast, shews that of the increased degree of heat generated by the more complete combustion, an increasing portion is occupied in the production of light.

17. The same truth is exemplified with additional force in the instance of a flame whose light is increased by the greater evolution of solid ignited particles, whilst its radiant heat does not sustain a proportional increase. If a particle of solid matter be volatilized into a flame of gas, the temperature of the gas is communicated to it; and the same temperature makes the solid particle give out much more light (estimated by its heating effect) than the gas, but not as much more heat. Therefore a portion of the heat communicated to the solid particle disappears as radiant heat, and is occupied in the evolution of light.

18. In the experiments on uniting different flames, the same thing is exhibited in a still more palpable manner. We there perceive that the simple heat radiated from two flames united, is much less than double that radiated from one. But yet, since by the junction of two equal masses of luminous matter at equal temperatures the heat must be doubled, it is evident that a portion of it is abstracted, and that at the expense of this portion the heating power of the light is increased.

We might argue in the same way from the experiments on incandescent metal. Since the total effect, or the values of $(l + h)$

must follow, (allowing for the nature of the determinations,) the same law as that of the cooling of the hot body, since those of (*h*) follow a less rapid law, we might infer that part of the heat was constantly abstracted, or ceased to appear in the form of heat; and this in proportion to the increased heating power of the light; the greater evolution of which thus contributes to the cooling process.

19. If it be admitted, as I conceive has been before shewn, that the whole heating energy of a luminous hot body is not displayed by the simple heat radiating from it; but though partly thus displayed, is also partly as it were communicated to another agent, through the medium of which it ultimately acts; this conclusion will receive still further confirmation from the arguments just adduced. From them we perceive that a continued and increasing disappearance of simple radiant heat always accompanies the increasing developement of heating power in the luminous rays.

20. I have adverted to the difficulties attending the supposition that the portion of heat which is not radiated in its simple form is *converted into light*; but since it is evidently abstracted, and afterwards appears again through a different channel, it will hardly be questioned that it is in some way employed in the extrication of the light. It, therefore, becomes important to inquire (so far as we can ascertain,) what is the real modification which it undergoes.

The facts teach us thus much:—This portion of the heat acquires totally different properties from those which it possessed either when in combination with the body from which it emanates, or which the radiated portion possesses. Though we have no right to *identify* it with light, it is yet evident that it is in some very close state of *union* with the light. It is so modified as to have lost its power of heating many sorts of solid matter; *transparent* bodies for instance; through which it is as it were conveyed, without any developement of its power of increasing temperature. Again it is not upon *all opaque* bodies that it can now exert its influence; and the degree in which that influence is exerted depends upon quite different characteristics in such bodies from those by which its ordinary influence is most developed. It accompanies the rays of light in their course, however altered,

and is never rendered sensible but when the light itself undergoes some change in its state. It disappears when light is formed or given off from bodies ; and re-appears when light is absorbed, or enters into combination. The principal part, at least, of the phenomena cannot be described as belonging to what we might call the *temperature* of light ; for then a black and a white surface would be equally heated by its impact upon them.

21. The relations of light and heat have justly been considered as involved in much obscurity ; but I cannot help thinking that the difficulties of the subject have been regarded as greater than they really are.

Of the nature of these two grand agents in physical phenomena we are completely ignorant, that is, we have not been able to detect any certain particulars in which they can be reduced by analogy to their place in the arrangement of bodies : from this cause it seems to have been tacitly taken for granted that since their *nature* is probably altogether *sui generis*, so their *mode of action* upon, or *connexion* with each other must be also equally beyond the dominion of ordinary natural laws.

To this inference, however, I think we are far from being necessarily led. With the powers of *light* as a chemical or physical agent upon the bodies, we are but little acquainted ; all that we know may be reduced to a few insulated facts. On the other hand, with respect to *heat*, the case is widely different ; not only do we recognise its action more or less in almost every phenomenon which nature presents, but nearly all the instances of its action have been reduced to general laws, and explained on regular theories.

Hence in attempting to investigate the relations subsisting between these two agents, it appears to me the course most likely to afford satisfactory information, that we should in the first instance take that of the two with whose effects we are best acquainted, and observing the laws of its connexion with ordinary matter, inquire whether any of those laws will apply to its connexion with light : this would be the course which on sound principles we ought surely to prefer before that of framing new suppositions to

account for effects which we suppose to be *sui generis*, only because we have not examined whether they are in accordance with any other class of phenomena.

22. What then are the most proper terms in which to describe the facts ?

The phenomena which light exhibits in its relations to heat, agree in the closest conformity with those presented by the changes of the ordinary forms of matter. When light is absorbed and enters into combination with other matter, heat is given out ; on the other hand, light is not generated or evolved without the application of a certain degree of heat. All bodies at some temperature become luminous ; and when they arrive at that point, a portion of the heat is employed in giving the form of light to some matter belonging to, or in combination with, the body *by becoming latent in it*.

23. To this view of the subject, I conceive we are directly, led by the foregoing experiments : the different results all afford a strong corroboration of each other ; and from them it appears, that instead of the vague opinion that " heat and light mutually evolve each other," we have it in our power to explain the phenomena which their union and separation present, in a way equally simple and satisfactory, and agreeing in the closest analogy with other physical phenomena ; and when the terms " absorption of latent heat," &c., are carefully used in their strict experimental sense, it is obvious that we do not, in applying them, necessarily assume the materiality of light, or of heat ; though, perhaps, the facts here brought forward may be considered as new arguments for it.

It would lead into details of too great length here, to proceed to the application of the principle above inferred, and to point out the ready explanation of many phenomena which it affords, and which, upon the received theories, are allowed to involve great difficulties. But it may be proper briefly to mention one or two instances.

24. The light from phosphorescent bodies is too feeble to allow us to ascertain whether it possesses any heating power when absorbed. In the absence of proof, analogy would lead us to suppose that

such a power must exist, though probably of very small intensity ; and the phenomena which these substances present, on the principles here advanced, agree in the closest analogy with other phenomena of nature. Some bodies, as water, mercury, &c., change into elastic fluids at common temperatures ; in the same way the common temperature of phosphorescent bodies may be sufficient to afford to some of their peculiarly-constituted particles, the requisite degree of latent heat, to evolve them in the form of light.

25. Phosphori, when exposed to light, absorb it, by which some degree of heat is necessarily communicated to them. This is in part occupied in causing the light to be again evolved, by becoming latent in it, as soon as the body is removed from exposure to light. An increase of temperature accelerates this process, and too great an increase soon exhausts the supply of luminifiable particles.

26. In general, I may further observe, it is on these principles easy to conceive, that there may be cases in which, for the evolution of light, no increase of sensible temperature should be necessary. The whole of such increase, though really generated, becoming latent in the light.

In general, also, the heating power of light evolved from different bodies, need not follow any proportion to its illuminating effect. It is conceivable that a body may produce light containing any given degree of latent heat, but of such tenuity, or so deep in colour, as to have very little illuminating intensity : of this we have seen an instance in incandescent metal.

I might proceed to the application of the principle here adverted to, in a variety of other cases ; but upon these topics it will not be necessary to go any farther. I will merely observe, in conclusion, that as Newton has put the query, whether light and common matter be not convertible into each other, we here perceive phenomena which render it highly probable that such a change actually does take place ; since, whatever the change be, it is accompanied by precisely the same phenomena in regard to latent heat, as those by which the changes of state, in ordinary matter, are accompanied.

ART. VII. *On Anhydrous Sulphuric Acid.* By ANDREW URE, M.D., F.R.S., &c.

DEAR SIR,

Several years ago I procured from a friend at Hamburg a bottle of the oil of vitriol of Nordhausen, intending at that time to examine its constitution, and to give you an account of it, in supplement to my paper on sulphuric acid, inserted in the 7th number of your Journal. This bottle was by accident overlooked by me till lately, when I made the following experiments on its contents.

A portion of this brownish-coloured oil of vitriol, sp. grav. 1.842, was distilled slowly from a glass retort, into a globular glass receiver, surrounded with ice. A white sublimate soon appeared in the globe and beak of the retort. When this sublimate was exposed to the air, it emitted a profusion of dense fumes of sulphuric (not sulphurous) acid. It burned holes in paper, with the rapidity of a red hot iron.

A phial, containing 988 grains of distilled water, which occupied two-thirds of its capacity, was poised in a sensible balance. Into this water a piece of the acid sublimate, of a tallowy consistence, slightly deliquesced on the surface, was dropped, and the phial was suddenly closed with its stopper, to prevent the ejection of liquid by the explosive ebullition which always ensues, when this solid acid comes into contact with water. The increase of weight due to the introduction of the acid into the phial, was found to be 56 grains. The total weight and specific gravity of this dilute acid were at 60° Fahr. 1043.6 grains, water being 1000. At this density, 1000 parts of sulphuric acid, of the common kind, contain 53 parts of dry acid, equivalent to 65 of liquid acid, sp. gr. 1.844. But in 1000 grains of the above dilute Nordhausen acid, there was at the utmost only 53.6 grains of the sublimate, which, considering the deliquescence on its surface, is a quantity agreeing with the supposition of its being anhydrous acid.

To put this point more fully at rest, I saturated 987 grains of the above dilute acid with pure carbonate of potash, prepared from

tartar, of which 90 grains were required. These 90 grains are equivalent to 51.5 grains of dry sulphuric acid. Now 987 grains of the dilute acid of Nordhausen, contained of the solid sublimate 52.9 grains, giving an excess of weight = 1.4 gr., due, most probably, to the adherence of moisture to the surface of the sublimate: 112 grains of dry sulphate of potash were obtained by evaporating the above saline solution. This portion of ignited salt contains 51 grains of dry sulphuric acid.

From the preceding experiments, it seems likely that the white sublimate from the Nordhausen oil of vitriol is anhydrous sulphuric acid.

I am, dear Sir,

Yours, &c.

Glasgow, March 1st, 1825.

ANDREW URE.

ART. VIII.—*Outlines of Geology, being the Substance of a Course of Lectures on that Subject, delivered in the Amphitheatre of the Royal Institution of Great Britain, by William Thomas Brande, F.R.S., Professor of Chemistry in the Royal Institution, &c.*

[This abstract of the above Lectures is published at the request of several of the Members of, and Subscribers to, the Royal Institution; they are intended to shew the objects of Geological Science, and to furnish the student with an outline which may assist his progress in the pursuit of the study. The general arrangement of the subject nearly corresponds with that adopted by Messrs. Conybeare and Phillips, in the first part of their excellent work on the Geology of England and Wales; from which, and from the invaluable collection of facts contained in the Geological Transactions, ample abstracts will be found in the following pages. To those works the geological student is especially referred, for such details as were inconsistent with the plan of a popular course of lectures upon so extended a subject]

I SHALL endeavour, in this preliminary discourse, to give a very brief outline of the origin and progress of geological science; to explain particularly the mode of pursuing it which it is pro-

posed, upon the present occasion, to adopt ; to shew the interest and the usefulness of the study in its various applications, as illustrating the natural history of our planet ; as unfolding those adjustments of inanimate nature which are calculated to display the wisdom of the creation ; as leading us to results useful in the arts of life ; and as propounding to the inquisitive mind an infinite variety of questions and speculations connected with the causes of the effects which we now perceive ; with the events which they announce as having happened at remote and obscure periods of the history of the Earth ; and with the various revolutions and changes which our globe seems destined to undergo, by the continued operation of the powers now active ; and by that perpetual warfare of the elements to which its surface is continually submitted. The bare mention of these, the genuine and legitimate objects of Geological science, naturally brings to the mind the awful and magnificent account of the creation, conveyed to us in scriptural history ; and geological writers have not unfrequently attempted to combine their speculations with the announcements of holy writ.—Mixing up the chronology of Moses and the history of the deluge with their own short-sighted speculations, and with observations hastily made and imperfectly reasoned upon, they have presumed, on the one hand, to verify and illustrate, and on the other, to question and controvert. But the arrogance of imperfect knowledge is nearly equally prevalent in both ; “ nothing,” says Lord Bacon, “ is more pernicious than to canonize error :” and again, adverting to the blending of natural philosophy with sacred writ, he calls it “ seeking the dead among the living,” and justly observes, that “ such vanity is so much the rather to be restrained and suppressed, as from the wild mixture of divine things with human, arise, not only fantastical philosophies, but heretical religions.” Far, therefore, from endeavouring to explain or controvert the arguments which have thus been by some annexed to, and blended with, geology, I shall altogether omit them, referring such as are interested in the legitimate part of the discussion to the masterly work of Mr. Granville Penn, entitled “ a Com-

parative Estimate of the Mineral and Mosaical Geologies,*" and I shall almost exclusively confine myself to the detail of those facts which lead to useful conclusions; hypotheses I shall not enlarge upon, because our time is very limited, and they rather amuse than instruct; and I shall only lightly touch upon such theories as are remarkable for their notoriety, or important from their connexion with, and illumination by, the leading facts of our science: but upon this subject, I propose to explain myself more fully in another lecture.

Geological writers may be divided, into those who are purely speculative; those who have built theories upon the examination of the structure of the earth's surface, or, at least, profess to do so; and those who, discarding speculation and theory, have contented themselves with the abstract detail of facts.

Of the former class, Dr. Thomas Burnet, who must not, as he sometimes has been, be confounded with the celebrated Bishop of Salisbury, with whom he was contemporary, stands pre-eminent. This writer, in his *Sacred Theory of the Earth*, which was originally published in Latin, between 1680 and 1690, and translated into English at the express request of Charles II., and which has been extolled for its eloquence and ingenuity by many of the most eminent authors, has taken a review of the past changes of the globe, contrasts them with those it is now undergoing, and foretells those which it is to suffer; and as the name of Burnet is continually occurring in geological history, it will not, I trust, be thought irrelevant, briefly to enumerate his opinions, more especially as this is the only time that I shall mention him or his doctrines.

In the first place, he ransacks scriptural and profane history, selecting from each such statements as suit his particular object, and endeavours to show that the primeval earth, as it arose out of elementary chaos, was of a form and structure different from that which it now exhibits, and so contrived as to contain within itself the materials necessary to the production of an

* See, also, a Review of Mr. Penn's work, in the fifteenth volume of this Journal.

universal deluge. He tells us, that when the elements separated from the original fluid mass, the heaviest particles tending to a centre, constituted a nucleus upon which water and air afterwards assumed their respective stations. The air, however, was not as we now see it a transparent attenuated medium, but it was loaded with exhalations and impurities which it gradually let fall upon the surface of the water, and then floated upon the whole in cloudless serenity. The deposited matter, constituting a rich crust, sent forth its vegetable productions, and soon became clothed with uninterrupted verdure; every thing was smooth, soft, and regular, and there was, he says, an universal spring; for the plane of the ecliptic was coincident with that of the equator. In process of time, however, the green and even surface just described, began to suffer from the continuous action of the sun's rays, which formed cracks and fissures that ultimately extended to the abyss of waters beneath, and these being sent forth by elastic vapours expanded by heat, soon inundated the superficies; an universal deluge ensued; and, in the violent shocks and concussions that attended it, rocks and mountains and all the inequalities of the present surface had their origin; then the waters gradually subsided into the residuary cavities forming the ocean, and partly were absorbed into the crevices of the disjointed strata and nucleus; vegetation began to re-appear, and the once uninterrupted and uniform surface was now broken up into islands and continents, and mountains and valleys.—Absurd, as from this condensed and unadorned sketch, Burnet's narrative must appear, it is told with such ingenuity and elegance, and supported with so much erudition, as to entitle it to all the merit that can belong to a highly elaborate and poetical fiction. Addison has eulogized it in Latin verse, Steele has praised it in the *Spectator*, and Warton, in his essay on Pope, ranks the author "with the select few in whom are united the great faculties of the understanding; judgment, imagination, memory."

But, although Burnet received and deserved the encomiums of the learned, the praise that he earned is rather that of the poet than of the philosopher. Dr. Flamstead, adverting to his rich

vein of poetical diction, told him "that there went more to the making of a world than a well-turned period;" and Mr. Warren, and Dr. Keill, of Oxford, each refuted and abused him as a theorist. Yet Burnet's work continued to be read, not for its philosophic truths or theoretic consistency, but for its splendid imagery, noble sentiments, and sublime conceptions.

Passing over several speculative authors, who, compared with Burnet, are neither instructive nor amusing, we meet in geological history with the writings of Woodward, who flourished in the latter half of the 17th century, and who must, I think, be considered as the earliest geological theorist who has professed minutely to examine the earth's external crust, and to found his opinions upon the results of its detailed inspection. In visiting the country about Sherborne, in Gloucestershire, he was struck with the variety of shells and marine productions visible in the strata; and he determined, with a degree of zeal which was then not very common, to undertake a geological tour, with a view of examining how far similar appearances were to be found in other quarries, and in remote parts of the kingdom. Having satisfied himself upon these subjects, and after registering in his journal a very copious account of his observations, he drew up a series of queries which he distributed amongst his friends and correspondents abroad; and, as the result of all his inquiries, he concluded that the earth's structure was not materially different in any part of the world, but that a general resemblance pervaded the contents and positions of its various beds and strata. In 1695, he published a work entitled "An essay towards a natural history of the earth and terrestrial bodies, especially minerals; as also of the sea, rivers, and springs. With an account of the universal deluge, and of the effects it had upon the Earth." This essay which is scarcely so much known as it deserves, excited a good deal of bustle amongst the philosophers of the period in which it was written; it was attacked, canvassed, examined, and defended, and called forth all those ephemeral answers, replies, and rejoinders, which flutter about controversy. Woodward did not confine himself to geology,

he attracted some notice as a physician, and more as an antiquary; and in his last will he founded a lectureship in the University of Cambridge; he died in 1728.

When we consider the untoward circumstances of Dr. Woodward's education, and the obstacles that in early life were opposed to the natural bent of his genius or inclination, we must allow him no small merit in encountering and overcoming them. At the same time, his life and writings are a good deal sullied by a peevish jealousy and visionary enthusiasm. He is ridiculed by Pope, under the name of Vadius, in his *Moral Essays*; and again in several parts of the *Memoirs of Scriblerus*, where an ancient shield, which the Doctor possessed, becomes the chief subject of the poet's merriment.

Among the correspondents and opponents of Woodward we meet with several authors whose works are never read, and whose names are falling fast into entire oblivion; there were others of more celebrated memory, and among them Leibnitz, who, towards the end of the 17th century, published his *Protogæa*, in which there is little more than crude and improbable speculations relating to the agency of fire upon a supposed chaotic mass. Nor are the geological opinions of Whiston deserving of more attention, though his work, published in 1696, entitled "A New Theory of the Earth, wherein the Creation of the World in Six Days, the Universal Deluge, and the General Conflagration, as laid down in the Holy Scriptures, are shown to be perfectly agreeable to Reason and to Philosophy;" gained him great notoriety.

It would, however, be an injustice to Whiston, were I to pass him by without quoting Locke's eulogium of his Theory of the Earth; who says, in a letter to Mr. Molyneux, bearing date Feb. 22, 1696:—"I have not heard any one of my acquaintance speak of it but with great commendations, and, as I think, it deserves: and, truly, it is more to be admired that he has laid down an hypothesis whereby he has explained so many wonderful and before inexplicable things, in the great changes of this globe, than that some of them should not easily go down with some men, when the whole was entirely new to all. He is one of those sort

of writers, that I always fancy should be most esteemed and encouraged: I am always for the builders who bring some addition to our knowledge, or, at least, some new things to our thoughts." But Locke's opinion upon such a subject, is not entitled to any especial weight. I have perused Whiston's book, without being able to find any particular merit in his speculations, and am rather inclined to side with his opponents, in forming an estimate of its value.

About this time a central fire was resorted to for the purpose of solving certain geological problems, by so many writers, that it is difficult to say to whom the merit, if such it be, of the invention belongs. To the views founded upon such a supposition, I shall advert more fully in another lecture; Beccher's *Physica Subterranea* deserves especially to be consulted in reference to this subject.

About the middle of the last century, geology began to assume a more regular scientific aspect; and among the earliest inquirers of this period, Mitchell and Whitehurst deserve something more than bare mention. The merits of the former writer have been overlooked, principally, I presume, on account of the title of his paper, which is in the *Phil. Trans.* for 1760, "On the Cause and Phenomena of Earthquakes," a title under which we should not perhaps look for geological, and still less for minute, practical information. We are chiefly indebted to Dr. Fitton for bringing him into notice, in an able article on English Geology, contained in the *Edinburgh Review* for 1811. This very ingenious writer describes the general appearances of the strata, points out their analogies and differences, adverts to their inclination and disturbance in mountainous districts, and to their horizontality in flat countries; and, having explained, with much minute and practical perspicuity, the arrangement of the strata in England, he exemplifies its universal application to the general structure of the globe; and ingeniously represents it in the following manner. "Let a number of leaves of paper," he says, "of several different colours, be pasted one on another; then, bending them up together into a ridge in the middle, conceive them to be reduced again to a level surface by a plane so passing through them as to cut off

all the part that had been raised; let the middle now be again raised a little, and this would be a good general representation of most, if not all, large tracts of mountainous countries, together with the parts adjacent, throughout the whole world. From this formation of the earth it will follow, that we ought to meet with the same kinds of earths, stones, and minerals, appearing at the surface in long narrow slips, and lying parallel to the greatest rise of any large ridge of mountains; and so, in fact, we find them.

Mr. Mitchell's paper abounds in important geological generalizations, and he applies his theories and inquiries with much dexterity and success to the structure of the whole surface of the globe, as well as to its individual parts.

Whitehurst is another writer of great merit in the history of English geology: in early life he passed a great part of his time in Derbyshire, a county well suited to excite and satisfy a mind endowed with the desire of penetrating into the formation of rocks, and into the origin and history of organic remains. The fruit of these investigations he submitted to the world in 1778, in his "Inquiry into the Original State and Formation of the Earth;" a work which, in its general outline and particular execution, does no small credit to the genius and diligence of the author. It is true, that much of it is tinctured by that unpropitious taste for cosmogony, which we have reprobated in preceding writers; but if we look to the practical details, we find them faithful to nature, and described with correct minuteness. To this point I need only quote the following passages. "The arrangement of the strata," he says, "is such that they invariably follow each other as it were in alphabetical order, or as a series of numbers. I do not mean to insinuate that the strata are alike in all the different regions of the earth, with respect to thickness or quality, for experience shows the contrary; but that in each particular part, how much soever they may differ, yet they follow each other in a regular succession."

In the writings of Mitchell, and of Whitehurst, then, we begin to discern something like a genuine and scientific investigation

respecting the structure, position, and contents, of the strata that envelope our globe. I have said nothing of Buffon, whose theories and speculations are much of the same cast as those of Burnet; nor of Rouelle, who reasoned more correctly, and discriminated with greater judgment; wishing to confine myself chiefly to the opinions, as well as to the writings, of our own countrymen. But in the works of this period one very important fact must not be overlooked, relating to the distinction insisted upon chiefly by Lehman, and some other continental authors, which may be made between rocks containing organized fossils, and those which are destitute of such remains; the former bearing evident traces of great revolutions and changes, the latter, apparently of an anterior date, and exhibiting no marks of animal or vegetable relics: the former, in the words of Lehman, owing their formation to partial or local accidents and derangements, the latter coeval with the world. To these which have since been called *secondary* and *primary* rocks, he added a third class, in which, as to position and structure, he professed to recognise the operation of the deluge; he presumed that they must have resulted from some great catastrophe, tearing up and modifying an ancient order of things. But in respect to the classification of rocks, it will be our business to speak more at length hereafter: and apprehensive that I may already have fatigued your attention with matters rather curious than useful, and with details historical rather than practical, I shall only further trespass upon it by briefly adverting to the speculations of two comparatively modern theorists, whose opinions have divided the geologists of our own time, and who may be called the founders of its once opposed schools; premising, however, that in their arguments and hypotheses, as in those of their predecessors, we shall find much that is blameful and faulty, mixed up with a body of practical and matter-of-fact information, of infinite use and value: indeed, though it is unfortunately my business to adduce many of these theories rather to refute, than to elucidate them; and though they are, with few exceptions, so many imaginary fabrics, displaying rather the fol-

lies than the genius of the wise, it must be remembered that to them we chiefly owe the practical foundations of geology.

It would be easy to shew that the theories, or rather hypotheses, of which we have now taken notice, contain the germs of those speculations and inquiries which in our own days have excited so much attention and controversy under the name of PLUTONIAN and NEPTUNIAN doctrines. The Neptunists affecting to trace all the present appearances of the globe to the sole agency of aqueous solution, disintegration, and deposition; and the Plutonists denying the exclusive operation of water, but combining its powers with those of fire, and calling in the aid of both elements.

The credit, such as it is, of the Neptunian theory, is commonly given to Werner; and if we find in it all the faults of his predecessors, and all the erroneous reasoning of darker ages, it will, at the same time be recollected, that to him belongs the principal merit of pointing out the order of succession which the various natural families of rocks are generally found to present, and of having himself developed that order to a considerable extent with a degree of accuracy which before his time was unattainable, for want of proper methods of discriminating minerals and their compounds. There is one disadvantage and difficulty attending an attempt to expound Werner's doctrines, which is, that we are obliged to take them at second hand, since he has not published any connected view of them himself; and moreover, the Professor and his scholars have generally affected a mysterious phraseology which it is very difficult to construe into common sense, or intelligible terms; and which is sometimes so harsh and uncouth as to border upon the ridiculous, when, at least, we attempt to put it into an English dress. The darkness which he has thrown round his doctrines seems, indeed, often as if it were expressly intended to keep them from the eyes of the vulgar and uninitiated, and may be compared to that mystic and symbolical language in which the alchemists delighted to veil the accounts of their researches, and which after all were no great things when by dint of much labour and study they were deciphered and done into plain and legible language.

In speaking, therefore, of Werner's theory, we can only avail ourselves of such transient glimpses as he has himself thought fit to give us, and must fill up the various chasms and breaks, with materials derived from the more extended and finished sketches and illustrations with which we have been favoured by his pupils and disciples. Werner's theory then amounts to this:—The matter of our globe was once in a fluid state, or at least its nucleus was enveloped by a chaotic solution of such a nature as to retain the various earthy bodies found in the lowest strata in chemical combination; but this state of things was of short duration, and the chaos began to deposit a variety of crystalline aggregates, such as the different species of granite, certain kinds of slates, or as they are more technically called, schists; the genuine kinds of marble, serpentine, and porphyry, and a few other more equivocal compounds. These constitute the *primitive* rocks or formations of the Wernerian school; they are supposed to have had their origin antecedently to the creation of living beings; they are more or less crystalline in their texture, and never contain any organic remains or rounded pebbles.

The second class of rocks included in this arrangement, are supposed to have been formed during the transition of the Earth from its chaotic to its habitable state. They are partly crystalline aggregates, and partly mechanical deposits; they contain fragments of pre-existing rocks, and are sparingly interspersed with imperfect remains of some of the lower orders of animals; certain dark-grey compact limestones, and the rocks called *grauwacke*, composed of fragments imbedded in a slaty paste, are the leading members of this family.

It is, then, imagined, that the elements acting upon these older rocks tended to their attrition and disintegration, and that several substances being mechanically diffused throughout the waters that covered the primitive and transition series were deposited upon them in successive layers, in a horizontal position. These are Werner's *Floetz* rocks; they not only contain, but even abound in, vegetable and animal remains; and among the latter, skeletons of amphibious animals are not uncommon.

Certain limestones, red sandstone, coal strata, lias, and chalk, and some other sandstones, belong to this division.

Lastly, we find depositions of sand, gravel, and clay, of the bones of quadrupeds, accumulations of peat, and some other substances now in progress of accumulation or deposition, which are included under the term alluvial formations.

The fifth class contains the produce of volcanic fires, and of more partial combustions. So that under one or other of these classes, or *formations*, as they are theoretically called, it is supposed that the various substances occurring amongst our rocks and strata may be included. It would be premature, to descant upon or criticise this theory, without more particular notice of the facts upon which it rests, and of those which militate against it, than I now can lay before you; but I think that you will hereafter, when in the possession of details, find me borne out in asserting, that it is, in all points, weak and unsatisfactory. That the very idea of an universal solvent, and of crystalline, succeeded by mechanical deposits, is at variance with all experience or analogy; in short, that it includes an unwarrantable accumulation of hypotheses, assigning opposite qualities to the same agent; and that, like most of its predecessors, it is equally at variance with nature, and with itself—"in a word, that it is a system which might pass for the invention of an age when sound philosophy had not as yet alighted on earth, nor taught man that he is but the minister and interpreter of nature, and can neither extend his power nor his knowledge a hair's breadth beyond his experience and observation of the present order of things."

The Plutonic theory, as it is generally though not quite properly termed, owes its origin to Dr. Hutton, and proceeds upon principles differing from those of the ancient Vulcanists, at the head of whom we may place Whitehurst, and entirely at variance with the leading tenets of the School of Freyburgh. It has been defended by the late Mr. Playfair, under the term of the *Huttonian Theory*, and his "Illustrations" rank among the most eloquent of scientific compositions; not but that he some-

times adduces doctrines which neither experiment nor analogy are competent to sanction, and which are rather adapted to delight the fancy than to convince the reason.

The Huttonian theory supposes the materials which compose the present surface of the globe to have been derived from the action of water upon a former order of things; that they are, in fact, the debris or ruin of ancient continents, which have been pulverized and worn away by the continuous operation of torrents and currents of water, which had transported them to the bottom of the ocean, and that there they have been consolidated by various causes, but chiefly by subterranean or volcanic fires; and we are to imagine the expansive power of the same irresistible agent to have again elevated the strata from the bottom of the ocean, to have given them various states of induration, and to have thrown them into those differing degrees of inclination to the horizon which they now exhibit; simply raising them in some instances; dislocating and removing them from their old posture in others; and occasionally effecting their entire fusion. The unstratified substances are supposed to have been in the latter predicament, while the stratified bodies are regarded as having been only softened by heat, or penetrated by melted matter. And as present continents were formed from the disintegration and corrosion of prior rocks, so are they supposed to be gradually restoring their materials to the sea, from which new continents are hereafter to emerge, manifesting a series of changes similar to the past.

Though in the details of these theoretical views there is very much that is fantastic and improbable, it must be allowed, that there is also much that is consistent with the known agency of bodies, and which is even directly borne out and verified by the actual result of experimental investigation. Indeed, the progress of modern chemistry has disburdened the Huttonian doctrines of some of their heaviest inconsistencies.

When facts and specimens are before you, I shall beg leave to call your attention to some of the chief elucidations of the Wernerian and Huttonian hypotheses; and then, and not till then,

shall we be able to make up our minds respecting their relative value and merits. I shall, however, dwell upon them, not so much from any conviction of their intrinsic importance, as with the intention of shewing the chief, and indeed real value of these and similar disquisitions, that of awakening in the mind a desire of investigating nature and of collecting facts: for observations accumulate very slowly when unassisted by the influence of system—the observer never proceeds with so much ardour as when he theorizes, and every effort to verify or to disprove particular speculations, necessarily leads to the evolution of new facts, and to the extension of the limits of useful knowledge. It is, therefore, the business of genuine philosophy rather to point out the imperfections, to expose the errors, and to restrain the presumptuousness of the theorist, than to attempt the entire extinction of a spirit which, however incomplete and insufficient the materials on which it has to work, must at least facilitate generalization, smooth the paths of knowledge, and render the approach to truth less rugged and tedious.

Having now attempted to bring before you a brief account of a few of the principal geological theories, with a view to give some insight into the objects of this branch of science, considered in the abstract, I trust I may recommend this pursuit, as embracing in its secondary applications, a variety of useful and popular information.

In mining, in the search for coal, in the structure of canals and roads, in building, draining, and in the judicious search for and management of springs, the advantages of practical geology are incalculable. It has frequently happened that materials for roads have been transported at great expense from distant parts, when they might have been abundantly procured in the neighbourhood; in sinking wells injudicious situations have often been pitched upon, and buildings erected at a distance from copious sources of water. In the fruitless search for metallic veins, millions have been expended upon the delusive promises of ignorant adventurers; and coal is frequently sought for in places, which even a slight knowledge of the subject indicates as hopeless.

The arrangement which I propose to adopt in the present course is too simple and obvious to need any detailed explanation. We shall commence with an examination of the superior or uppermost strata, and more particularly examine into that wonderful history of the devastation and destruction of a former order of things which is exhibited by the various alluvial and diluvial matters that cover the uppermost of the secondary strata.

These secondary strata we shall then examine in their natural succession—the chalk, with its contents and accompaniments will claim much of our notice ; and here, and in the superincumbent beds, we shall find brick, earth, potters' clay, and various other products applicable to the arts ; here, too, we shall discover the seat of those subterraneous rivers, which, flowing from more elevated situations, make their appearance at different depths, and in different strata, constituting the various springs of hard and soft water about the metropolis.

The next substances that occur, are the varieties of freestone used in rough sculpture and building, such as those of Bath and Portland, with their various argillaceous concomitants ; and to these succeed the red sandstone and marl strata in which are our deposits of rock salt, and with which are associated the still more important formations of coal and iron-stone ; the whole of these strata covering a vast extent of country, lie in enormous cavities or basins bounded by that variety of marble, commonly called mountain limestone, in which, in Derbyshire, and several of the northern counties, are also various subterranean treasures, and whence, more especially, are derived the enormous supplies of lead which enrich the British market. The truly metalliferous formations, or those rocks and strata in which the veins of the great mining districts of England occur, will now be exposed to our view ; and we ultimately arrive at granite and its associates, which form the great and primæval mountain chains of the world, and upon which, as far as our limited inquiries enable us to ascertain, all the other rocks are incumbent.

Having thus examined the strata of our globe in the order of their succession, from the surface downwards, I propose to de-

vote some lectures to the structure, position, contents, and theories of metallic veins; to a general account of the relative durability of rocks and mountains; and to an examination of the causes which are tending to their disintegration and decay—connected with this subject is the nature and formation of soils, and some other topics, which may prove of general use and interest.

I shall conclude my course with some observations connected with the causes and effects of volcanic fires, with the phenomena of earthquakes and boiling fountains, and with some remarks relating to other limited phenomena, as far as they tend to elucidate or explain any mysteries and difficulties which we may have met with in our previous inquiries.

Hastily as we have now gone over the subjects of geological research, I shall consider my intention in this introductory discourse as amply attained, if I have convinced you that it is no unworthy or unimportant branch of science; but that, on the contrary, it embraces enticing, useful, dignified, and even sublime materials of discussion and instruction.

It surely concerns us all to know something of the ground we tread upon, of the country we inhabit, and of the sources, and natural association of the infinitely-varied products, with which the mineral world assuages our wants, increases our comforts, and multiplies the luxuries of life.

To the traveller, geology opens, as it were, a new creation; in connexion with geography, it discloses what may be termed the physiognomy of the earth; thus clothing a barren country with numerous objects of interest, and giving a new zest to those pleasing emotions of the mind with which we behold the fertile landscape and fruitful plain; and to those more exalted sensations which are created by the majestic features of a rocky and mountainous district.

Finally, in displaying to us, as it does, in characters most unequivocal, the great and awful revolutions which this earth has suffered, it gives rise to a salutary reminiscence of those which yet may come; it shows us upon what slight foundations the pre-

sent order of things rests, and how trivial a change would suffice to obliterate all that now exists ; thus inspiring the well-attempered mind, not with sorry fancies and idle fears, nor with that superstitious awe which sometimes results from gross ignorance, and sometimes from perverted knowledge, but with deep and unshaken admiration of that boundless wisdom which governs the revolutions of nature—which,

Builds life on death, on change duration founds,
And bids the eternal wheels to know their rounds.

II.

HAVING noticed some of the leading subjects of geology, and briefly enumerated some of the hypotheses entertained by our precursors in this department of physico-chemical science, I shall proceed more particularly to explain its objects, as they are now pursued, and endeavour to sketch an outline of the study, as it exhibits itself at the present day.

Its first and leading object is to become practically acquainted with the present state of the Earth's external structure ; for, excepting of its crust or rind, we know nothing ; and all that has been suggested, either by theory or experiment, relating to its internal *composition*, its density, and the constitution of the entire mass, is mere surmise and guess-work—deductions hastily drawn from superficial observation, or unwarranted inferences from imperfect researches.

The present surface of our planet is composed of lapideous materials, the nature and composition of which, it is the business of mineralogy and of chemistry to determine ; not that the minutiae of either of those studies need, of necessity, be gone into by the geologist, for the substances which thus present themselves are, comparatively, few in number and simple in their nature, and their external characters and intimate composition are soon learned ; yet, if he would pursue his subject under every advantage, and extend his inquiries into its more refined departments, he must neither be a superficial mineralogist, nor an imperfect

chemist. A knowledge of the crystalline forms, and general mechanical characters of substances must often be called to the aid of his speculations, and he can frame no plausible theory without combining with such information a just, and even minute, acquaintance with the effects of heat and various solvents upon mineral masses, and upon the parts of which they consist.

Without these guides, therefore, he who aims at any thing beyond mere practical geology, will infallibly go astray: they are lights essential to his successful progress, and the wanderings of those who would penetrate into the more secret parts of geology without them, are abundantly manifest to the intelligent reader, in most of those speculative writers, whose eloquence and dexterity in argument may mislead the unwary into an acquiescence in their reasonings, but which, when measured by less fallible standards, are found void of solidity and truth.

Siliceous, calcareous, and argillaceous substances, either pure or nearly so, and in a state of mixture, or loosely and indefinitely blended, rather than in strict chemical combination, constitute a very large relative proportion of those rocky masses, or scattered or comminuted substances, which form, or have formed, the most exterior constituents of our planet; and of these, considered in the abstract, the chemical and mineralogical history is soon told.

Under the name of *rock crystal*, or *quartz*, SILICA presents itself nearly pure; and in the varieties of flint, agate, calcedony, and siliceous sand, it is, by far, the predominating ingredient. Its most usual crystalline form is a six-sided prism, terminated by a six-sided pyramid; but its primitive form is an obtuse rhomboid, nearly approaching the cube. In specific gravity the pure varieties fluctuate a little on one side or other of 2.6. It cleaves with great difficulty; its common fracture is conchoidal. There are many varieties as to form and colour, which chemical mineralogists describe and distinguish; they are all hard enough to scratch glass. The leading *chemical* characters of silica are, extreme difficult fusibility; insolubility in water, and in nearly all acids, except under certain peculiar circumstances of recent

precipitation, under which it readily dissolves in potassa and soda; it is also easily combined with those alkalies by fusion, such compound constituting the basis of glass, and where the alkali greatly predominates, being soluble in water. This solution is called, sometimes, *liquor of flints*; the acids decompose it, and throw down a finely-divided *hydrate of silica*, which is soluble, to a small extent, in water; a fact of some importance in relation to our present subject, and accounting, in some measure, for the existence of silica in certain mineral waters, and more especially in those of the boiling fountains or geysers of Iceland, which deposit it in abundant incrustations. Calcedony, and some other minerals, are also occasionally referred to, as attesting the once fluid state of silica from a watery solvent: its crystalline forms may have originated from igneous fusion, but the existence of aquatic *confervæ*, often of their native colour, or discoloured by oxide of iron, and of mosses and lichens, in certain agates, renders fire, in many instances, an inadmissible agent. Upon these subjects, Dr. Mac Culloch's inquiries are extremely deserving notice. Even the chrysalis of a moth is, it is said, extant in agate. One other chemical character of silica must not be overlooked, as bearing upon geological theory, which is, its affinity for other earths, either in igneous fusion, or aqueous solution. In the former way, it combines with lime and alumina, with magnesia, baryta, and strontia. Pottery and porcelain are alumino-siliceous compounds, with an occasional proportion of magnesia. Three parts of silica, 2 of magnesia, and 1 of alumina fuse into a glass; and we shall, by and by, find several compound minerals of analogous constitution. Nor must the humid attractions of silica be overlooked, for when its alkaline solution is added to aqueous solutions of lime, baryta, or strontia, or to an alkaline solution of alumina, compound precipitates of the earths are the results, several of which have been particularly examined by Mr. Dalton. (System, p. 841.) It may be right to add, that silica is, probably, a binary compound of oxygen with a peculiar inflammable base, which has been called *silicon*, and that it appears to contain about half its weight of

oxygen; but no application of this subject to geology renders it necessary to dwell upon it, however interesting as a branch of chemical inquiry, and as shewing how great a proportion oxygen constitutes of the solid, as well as of the fluid, matter of our globe.

CARBONATE OF LIME is a very predominant ingredient in rocks—marble, chalk, oolite, freestone, and all the purer varieties of limestone, are essentially composed of it, and these substances form strata of prodigious extent. It predominates in all the varieties of shell, coral, and madrepore: with other substances, but especially silica and alumina, and a little oxide of iron, it is found in lias, and several marls and clays. Calcareous spar and statuary marble may be selected as this substance in a very pure state. The former, when transparent, is highly doubly refractive, as we see in Iceland spar. Its crystalline forms are very various, but they all result from a *primary rhomboid*, the angles of which are $105^{\circ} 5'$ and $74^{\circ} 5'$. Exposed under ordinary circumstances to heat, it loses carbonic acid, and leaves quick lime, and by such an experiment its composition is shewn to be 28 lime + 22 carbonic acid, or *per cent.* 56 and 44: but if it be exposed to heat and pressure, so as to prevent all escape of gaseous matter, it then fuses, and retains its carbonic acid; a fact of no small importance as connected with certain theoretic considerations of the Huttonian school of geology. Carbonate of lime is easily recognised by its softness, and by the effervescence which it produces when a little dilute muriatic acid is dropped upon it; and as none of the other carbonates constitute mineral masses, this criterion is alone sufficient to distinguish it under such and several other circumstances. Or, the presence of lime in solution may be detected by oxalate and by carbonate of ammonia, which, added to muriate of lime, occasion precipitates of oxalate and of carbonate of lime; both these yield quick-lime upon exposure to a sufficient red heat.

ALUMINA rarely occurs pure in nature, but it is very abundant in conjunction with other earths, and is present in all clays and *plastic* earthy compounds, on which it confers the property of

exhaling an earthy odour when breathed upon. It is nearly infusible when pure; but, as has already been stated, it enters into combination with the other earths, so as to produce fusible compounds. When in a very comminuted state, it dissolves in the caustic fixed alkalies, and in most of the acids;—with potassa and sulphuric acid, it forms the characteristic octoedral crystals of common alum. Alumina has so strong an attraction for water as to retain it for some time, even at a red heat. In its pure and crystalline form, it constitutes the gem called *sapphire*, of which there are several coloured varieties.

MAGNESIA, like lime, forms a part of several compound minerals, most of which have a greenish colour, and a soapy feel. Mixed in the state of carbonate, with carbonate of lime, it is found in a subspecies of limestone, to which it imparts peculiar characters. It is insoluble in the alkaline solutions, but readily soluble in many of the acids; and its presence is announced by a precipitate of carbonate of magnesia, on adding carbonated fixed alkali to its solution; while, on the other hand, bi-carbonate of ammonia, which throws down lime, does not precipitate magnesia unless phosphate of soda be added; and upon this is founded an elegant method of detecting the presence of magnesia, devised by Dr. Wollaston. Now the different sands, clays, marls, and limestones of the upper strata, are composed of mixtures of the substances just enumerated, and they constitute, in chemical combination, and with a few unimportant additions, the principal components of the primary rocks.

The regular succession of the Earth's strata has been already partially referred to, as determined by the inquiries of some of the earlier geologists; and as the order of this succession, and the respective characters of the series, will occupy much of our attention, it may be right to explain it by a few preliminary observations. Let us suppose a traveller, for instance, departing from London and travelling westward towards North Wales; and let us direct his attention to the ground which he passes over—he first traverses a tract of clay and sand, then he enters

upon chalk, which is succeeded by calcareous freestone and a species of argillaceous limestone; then comes a zone of red sandy marl, and mines of coal and iron succeed, surrounded by limestone, and followed by slate and granite.

If, instead of proceeding westward, his route lie to the north, the same succession of strata, the same rocks and mineral masses, exactly in the same general order, will present themselves, and various opportunities will occur to lead him to another important fact respecting these stratified beds, which is, that they are evidently disposed at an angle, and neither vertical nor parallel to the horizon; so that their edges may be observed successively to emerge from under each other. The term *outcrop* or *basset* of the strata is applied to the successive zones thus formed, and their inclination or dip is found subject to much variation.

It is impossible to contemplate this arrangement without discerning the important secondary purposes to which it so efficiently contributes; for those strata which at one place are at impenetrable depths, are at another so brought to the surface, as to enable us to examine and obtain them and their contents: numerous useful products and mineral treasures are thus collected at, or comparatively near, the surface, with which we otherwise must have remained wholly unacquainted, or which could only have been procured by almost insurmountable labour and expense. Without such obliquity of stratification, there would have been no succession of soils. "In the whole machinery of springs and rivers, and the apparatus that is kept in action for their duration through the instrumentality of a system of curiously constructed hills and valleys, receiving their supply *occasionally* from the rains of heaven, but dispensing them *perpetually* in thousands of never-failing fountains," we see other important consequences of this arrangement of rocks. Waters, collected upon the hills and on high ground, filter and flow through the softer and permeable layers, producing springs in the valleys, and feeding streams and rivers, instead of accumulating in marshes and swamps as they would do, were the strata horizontal, or the surface plane.

Among the strata or formations of the vicinity of London,

chalk forms a very conspicuous feature; but there are, lying upon it, a variety of clays and sands, and beds of gravel, which will claim our earliest notice. Beneath the chalk, another and a distinct series of sands and clays will be found to prevail, which are incumbent upon calcareous freestone and lias: to these succeed marls and sandstones deeply tintured by red oxide of iron, and often containing detached masses, as it were, of rock salt and alabaster. These may be considered as constituting one comprehensive and important subdivision of the strata. The next includes the coal deposits, and the sandstone and limestone with which they are more immediately associated, or upon which they are incumbent. The third subdivision is characterized by the prevalence of slaty or schistose rocks; and the fourth is confined to granitic aggregates, of which there are many sub-species.

We thus find the coal strata interposed between two great families of rocks, which, with Messrs. Conybeare and Phillips, we may call the *supermedial* and *submedial* orders; coal and its associates are the *medial* order, and granites on the one hand, and alluvial and diluvial matters on the other, form the *inferior* and *superior* series. Using these terms, we lose sight of all theoretical distinctions—of primary, transition, and secondary formations.

The medial order of rocks, and all above them, are characterized by containing remains of vegetable and animal tribes; and these are sometimes in great profusion; but they are far from being indiscriminately scattered through the strata; on the contrary, “they are disposed as it were in families, each formation containing an association of species, peculiar in many instances to itself, widely differing from those of other formations, and accompanying it throughout its whole course; so that at two distinct points on the line of the same formation, we are sure of meeting the same general assemblage of fossil remains.”

In the carboniferous limestone, for instance, forming, as we have said, a member of the medial series, we always find the same corals, encrinites, terebratulæ, &c., from whatever part of the world our specimen comes. In the chalk, too, there is an asso-

ciation of shells, echini, and so on, peculiar to it; but if we compare the relics in the mountain limestone with those of the chalk, we shall find that they, in all cases, are perfectly distinct; that there is not, even in any single instance, a remote resemblance or analogy between them. Even between contiguous beds, there are often, in this respect, very striking distinctions, and the whole subject is of so singular and problematical a nature, as to have attracted, in an especial manner, the attention of the geological theorist. We have, for instance, as the lowest crust of the globe, or as its *nucleus*, as some mechanical philosophers will have it, a compound crystalline rock, generally very hard and permanent, in which the most inquisitive eye has as yet traced no organic remains of either kingdom of nature, no rounded pebbles, or any relic or detritus of other rocks. Symptoms of disturbance, perhaps of fusion, it does, indeed, exhibit, as our specimens will hereafter teach us. On this granitic foundation is reared a slaty structure; and in the rocks between it and the coal formations, animal remains, characteristically different from any which now exist, begin to make their appearance, and become abundant in the carboniferous limestone. Rounded pebbles, and other proofs of the existence and destruction of former rocks, are also here met with; but, what is not a little remarkable, we find in the coal strata themselves, which thus repose on the coral limestone, scarcely a single shell or relic of the kind; but on the other hand, abundance of vegetable remains, all, I believe, of unknown species, but of genera often allied to plants that now inhabit tropical climes. In the magnesian limestone, resting upon the coal measures, marine remains are again frequent; but in the red marl, which next follows, scarcely a shell or plant of any kind occurs. In formations that intervene between the red marl and the superficial strata, that is, in the varieties of lias and freestone, in the ferruginous and chloritic sands, and in the chalk itself, we find peculiar corals and echini, the remains of testaceous and crustaceous animals, of marine oviparous quadrupeds, and of vertebral fishes, all of extinct species, but quite distinct from those which we found below the red marl, and often remarkably distinguished among

themselves according to the individual strata which they occupy. Lastly, in the uppermost and superficial strata, we find shells, not, as in former cases, changed, petrified, or lapideous, but in such a state, that when washed and cleaned, they might pass for recent ; some of these correspond to marine shells, and others, in other strata or beds, resemble our present fresh-water shells ; whence the alternate inundation on certain spots of fresh and salt water has been inferred. In the most superficial and highest of these strata, we at last arrive at shells such as now exist, and then comes that great tell-tale of the deluge, the gravel, in which we discover the remains of land quadrupeds of unknown genera, and of extinct species, and, commonly, the latter correspond to inhabitants of very different climates at present.

Another important circumstance upon which I have only slightly touched, is the very frequent occurrence of rounded pebbles composed of fragments of the older rocks, which are abundant in the secondary strata. These pebbles prove the antecedent formation and consolidation of the rocks of which they are the debris ; and they also shew another curious fact in the history of stratification, which is, that where they occur, as they often do, in vertical beds, they must have acquired that verticality by the operation of some force which has thrown them out of their original horizontal position ; for we cannot suppose it possible that any extensive accumulation of loose gravel can have taken place upon surfaces greatly inclined to the horizon.

Such beds of consolidated gravel are common in the red sandstone below the carboniferous series, in that series in the lower strata of the superior red sandstone, in the sand below the chalk, and in the gravel immediately above it and below the London clay.

Now it is impossible to contemplate the collection of marine relics which are so abundant in many of the strata, and which are found upon the summits of some of the loftiest mountains of the world, without immediately coming to the conclusion that our present land has not been merely transitorily covered by the waters of an inhabited ocean, but that it has actually been

formed of materials collected through a series of ages in the bosom of an aqueous abyss; and one of the first problems we are called upon to solve, is the cause of the change of level in this ocean, and of the emersion of our present land. It is difficult to conceive any considerable change in the actual quantity of water, resulting either from its transmutation or decomposition; yet such causes must not be excluded as impossible. But the most obvious solution of the difficulty is founded upon the supposition that certain powerful agents have elevated our present continents, and at the same time depressed the bed of the ocean; what was once, therefore, the bottom of an antediluvian sea, now appears to be our habitable land; and perhaps the dry land of a very remote period of the world, may be the bottom of the present sea. This disruption and elevation of the strata is not by any means a mere gratuitous assumption, for independent of the verticality of loose gravel beds, and other highly inclined strata which must have been once vertical, we have so many other instances of dislocations and heavings up of the strata among the old as well as among the newer rocks, as, in the opinion of some, to demonstrate to the utmost certainty the agency of an irresistible elevating force; but here the inquirer will perhaps stop to ask *what force* or *what power* in nature could have been adequate to such extraordinary and gigantic effects; a question which must be answered by minute inquiry into the position of the strata, their associations, texture, and derangements, and by a comparison of these with powers and causes now active or subject to our control and investigation.

Another geological consideration, independent in a manner of the former, but of much interest, and of some difficulty, relates to the causes which have been active in producing the present *more superficial appearances* of the earth; in carving it out into valleys, and fitting it as it were for the order of things which now prevails.

Valleys are more or less extensive furrows of the surface, ramifying generally to a considerable lateral extent, and independent of secondary purposes, fulfilling that most essential one of drain-

ing the adjacent lands of their water, and carrying it in brooks and streamlets, which gradually unite to form rivers, and ultimately convey their contents into the ocean, of which, in fact, they constitute a series of ascending branches.

Now there are some valleys and river channels which appear to owe their origin to some great convulsion or catastrophe, which has torn asunder the rocks and strata, and left certain intervening chasms; but in by far the greater number of instances we have no evidence whatever of the activity of such agents, and every thing indicates a less sudden and violent, but yet an energetic, cause.

Valleys frequently intersect the strata which they traverse, in such a way as to leave no doubt of their subsequent formation to that of their bounding rocks, a phenomenon almost always observable, and sometimes within very narrow limits; as where in a mountainous country a rapid river cuts through a narrow defile; on these occasions we observe the same strata repeated upon each wall, as it were, of the valley, the soil or substratum of which consists of the lowest layer of the series. Now when we witness such appearances, and when among the pebbles, and sand, and fragments of the low-land we find comminuted portions of the surrounding rocks, when we discover a collection of such debris in the soil and bed of the river, we almost necessarily arrive at two conclusions; one, that the strata once were continuous; and the other, that the intersecting agent has been water: water, not flowing as it now does, quietly through the valleys, but constituting mighty and destructive torrents, which, while they have intersected, have at the same time, in many instances, denuded the strata; which sometimes working upon soft or muddy materials, have transported them to a distance, but which have also ground down the hardest substances, and aided by the gravel and debris thus formed and impetuously hurried along, have chiselled out furrows in the more indurated and primary rocks. Some have vainly imagined the present streams as adequate to the production of such effects; but he whose sight is not dimmed by hypothetical blindness, will see evidences of a more powerful agent, and will at

once, and plausibly fix upon the deluge. I leave the proofs of such irresistible and extended currents to future lectures; but of their general agency, the instance of intersection and denudation to which I have alluded affords strong confirmation; for it sometimes happens that in insulated hills, in *outliers*, as geologists sometimes call them, we discover, although at a considerable distance from the chain with which they were once connected, some of the lower ranges of those strata, the upper ones having been washed away.

But although we find it necessary to refer, for satisfactory explanation of much that we now see, to that period when the earth "was covered with the deep as with a garment," and when "the waters stood above the mountains," it is by no means intended to exclude altogether the influence of later and partial inundations, of which we shall duly take notice, but which are manifestly inadequate agents to the production of the effects just mentioned. Hence the necessity of a distinction of the terms applicable to them, and the propriety of designating the water-worn fragments, and pebbles, and debris of all the rocks, by the term *diluvial* products, while we limit the term *alluvium*, to the sandy and muddy collections of our present currents: these are seldom carried far; they form flats and deltas near the mouths of rivers, and accumulations of finely-divided matters in their beds; but of the comparatively gigantic force of diluvial currents ample testimonies are extant; and when we find pebbles which have obviously been thus transported for many miles, and even huge blocks which have suffered a corresponding transplantation and attrition, and lodged in valleys separated by lofty ranges of hills from the rocks whence they must have had their origin, we may not only form some idea of the force of the torrent, and the power of attrition of the substances impetuously hurried along by it, but we also read in such phænomena the most unequivocal evidence of the non-existence of many of the deepest valleys and most striking irregularities of surface which now exist, at the time that the boulders and pebbles were transported.

Several geological hypotheses, and the Huttonian theory in particular, affect to descry, in the agency of existing causes, the

source of those effects which we have referred to extraordinary and occasionally acting forces ; they have assumed the present rivers as the excavators of their own valleys, and ordinary volcanic fires as the indurators and elevators of strata from the bosom of the deep ; they think that the washing down of finely-divided matter formed by the action of air and water upon the present surface, and the inroads of the ocean as manifested by the abrupt precipices and shingles of the beach, are sufficient evidence of the slow but sure destruction of the present order of things ; but such actions are not only of very circumscribed extent, they are absolutely inefficient ; and even if we draw unlimitedly upon *time*, as the theorists, in opposition to all chronological evidence, have done, their tendency is often the very reverse of that which they would substantiate. The fact is, that the wasting and wearing causes that now exist, are either too trifling to be taken into the account, or are counterbalanced by renovations and re-accumulations ; in proof of this the barrows of the aboriginal Britons have been well adduced by Mr. Conybeare *, as retaining the pristine sharpness of their outline, after a lapse of little less, and probably more, than two decades of centuries : even the fosse that surrounds them is not filled up. “ Causes, then, which in 2000 years have not, in any perceptible manner, affected these small tumuli, often scattered in very exposed situations upon the crests of our hills, can have exerted no very great influence on the mass of those hills in any assignable portion of time which even the imagination of a theorist can allow itself to conceive ; and where circumstances are favourable to a greater degree of waste, still there is often a tendency to approach a maximum at which further waste will be checked ; the abrupt cliff will at last become a gentle slope, and that slope become defended by its grassy coat of proof.” Even the ocean itself throws up shingle banks and marsh lands, which check its further inroads. So that these supposed destructive powers, as they seem to the superficial observer, or to the biassed theorist, are soon found to

* Conybeare and Phillips: *Outlines of Geology*, page xxxii.

neutralise themselves, and are certainly inadequate as general agents, though their occasional and local effects may, if not duly weighed and compared, mislead us in the estimate of their powers. Indeed, of accumulation rather than of decay, of growth rather than of wasting away, we have further remarkable illustration in the phenomena of submarine and subterranean forests of trees manifestly buried in their natural position, as on the coast of Lincolnshire, Pembrokeshire, and Lancashire, in the valley of the Thames near Purfleet, and elsewhere. We also have instances of the agglutination of sand into sandstone, on the north coast of Cornwall; of æstuaries filled up by alluvial debris, as in the Cornish stream-works; and many other attestations of the extension rather than the destruction of habitable surfaces.

Having now sketched the business of the succeeding lectures, and having briefly enumerated the various theories which it will be my object more fully to discuss and explain, I shall proceed to examine, in detail, the most superficial strata of the globe, and to pursue that plan of geological inquiry of which the heads have been enumerated.

(To be continued.)

ART. IX.—*On the Hygrometric Properties of Insoluble and Difficultly-soluble Compounds.* By Mr. T. Griffiths.

[Communicated by the Author.]

THE power of absorbing moisture from the atmosphere has been shewn by Professor Leslie not to be peculiar to acids, alkalies, and deliquescent salts, but to be also possessed by several insoluble chemical compounds.

His experiments on these are not, however, very numerous, and are all chiefly in reference to the degree of dryness they indicate upon the hygrometer, when submitted to the action of air saturated with aqueous vapour. The subject appearing one of con-

siderable importance, and likely to offer many new and useful results, a more extended series of experiments were undertaken, with the view of determining the actual increase, sustained by given weights of various insoluble bodies, on exposure to a very damp atmosphere.

Portions of the various oxides, chlorides, and salts of earths and metals, were reduced to fine powder, and dried on a sand bath, till they ceased to deposit moisture upon a cold glass plate, held over their surfaces; 100 grains of each were then accurately weighed, and placed in small glazed earthenware pans, about three inches in diameter, and a quarter of an inch deep; these were all respectively numbered, and a list of their contents provided. The substances being thus far arranged, were placed in parallel rows at the bottom of a shallow box, which was now taken into a small out-building, about six feet square, the atmosphere of which was saturated with moisture. This was completely effected by covering one of its sides with flannel, kept constantly wetted from a vessel of water placed near the roof of the building.

The cover of the box was slightly raised, so as to allow free access of air, and at the same time exclude any particles of dirt or dust that might otherwise have fallen on its contents.

Upon making an experiment with an hygrometer in this atmosphere, the temperature (45°) closely coincided with the dew point; thus proving its saturation with aqueous vapour. Under these circumstances the various substances remained for a month, during the whole of which time the atmosphere was kept saturated; at the expiration of this period the temperature of the air and dew point being 37° , they were again all accurately balanced, and their increase of weight carefully noted. These results, together with the names of the substances employed, are given in the first of the annexed Tables .

Spongiform silver, powdered bismuth and antimony, of each 50 grains, dried, and exposed with the above substances, acquired no increase in weight. A similar quantity of spongy platinum gained however .1 of a grain.

It is well known that newly-made charcoal has the property of

absorbing air and moisture with such avidity, that it can hardly be removed from one vessel to another, without increasing in weight. This takes place in charcoal from different woods, with various degrees of force; the second Table shews the increase in every hundred parts of charcoal from thirteen different woods, weighed whilst very hot, and exposed for a week to the above atmosphere.

Oxide of zinc		29.
————— chrome		10.
————— iron (colcothar)		3.1
————— manganese (black)		2.5
————— lead (litharge)		1.7
————— bismuth		.7
————— iron (scales)		.5
————— lead (red)		.2
————— mercury, by nitric acid		.2
————— copper (black)		.1
————— tin (putty)		.1
Chloride of silver		.6
————— lead		.5
Sub. mur. of copper		1.8
Chromate of lead		.5
————— mercury		.1
Sulphate of lime		16.2
————— lead		.4
————— baryta		.3
————— strontia		.1
Sulphuret of antimony (liver)		.9
————— antimony (black)		.4
————— mercury (cinnabar)		.4
Bi-sulphuret iron		.2
Phosphate of lead		.5
Tartrate of lead		.7
Plumbago		4.5
Carbonate of lead		.6
————— zinc (calamine)		.5

Hygrometric Properties of Insoluble Compounds. 95

Carbonate of lime (chalk)		.8
———— baryta (native)		.2
———— strontia (native)		.1
Aurum musivum		.2
Clay iron stone		.5
Smalt		2.1
Fluor spar. Blue variety		.4
Cornish clay		2.4
Serpentine		5.2
Mica slate		1.1
Drawing slate		1.
Zeolite		.3
Granite		.2
Silica—powdered quartz		.2

Charcoal from walnut-tree		17.3
———— tulip-wood		15.4
———— ash		15.3
———— Botany-bay wood		15.2
———— launce-wood		13.7
———— cedar		13.4
———— America pine		12.6
———— willow		12.1
———— birch		12.
———— rose-wood		12.
———— lime-tree		11.8
———— king-wood		11.5
———— Zebra wood		6.6

Foolscap paper		18.2
Cartridge		17.1
Brown		15.3
India		11.6
Filtering paper		5.

Known weights of these various specimens of paper, dried strongly before a fire, were exposed to the atmosphere for 24 hours, and gained the annexed increase for every 100 parts.

The respective quantities of moisture, absorbed by all the substances, may very probably vary according to the method of their preparation and state of mechanical division, but as far as general results are concerned, they will not differ very widely from the above.

ART. X.—*On the Production and Nature of Oil of Wine, (Oleum Æthereum of the London Pharmacopœia,) by Mr. H. Hennell, Chemical Operator at Apothecaries' Hall.*

[Communicated by the Author.]

MR. R. PHILLIPS, in his translation of the London Pharmacopœia, appearing to doubt the existence of Oil of Wine, as a distinct substance, I was induced carefully to repeat the process we usually adopt in our laboratories for obtaining it.

Half a gallon of rectified spirit of wine (Sp. Gr. 830,) was mixed with an equal bulk of sulphuric acid, and distilled in a glass retort; the products were ether, water, sulphurous acid, and about four ounces of a yellow fluid floating upon the water, which, when separated and washed with solution of carbonate of potash as long as there was any trace of sulphurous acid, was a solution of true oil of wine in ether. The ether may be removed, either by spontaneous evaporation, or it may be distilled off with a very gentle heat. The oil thus obtained, and which amounts to about two ounces, is a yellow fluid, resembling, in appearance, oil of lavender or peppermint; perhaps rather more viscid. It has a specific gravity of 1.05. After being kept a few months, it becomes more viscid, and a number of prismatic crystals form in it, which, in many of their characters, very much

resemble Naphthaline ; they are soluble in ether and alcohol, and crystallize from both those solvents in very slender prisms ; they melt with a very slight heat, and sublime unaltered ; in warm sulphuric acid they dissolve, forming a pink solution ; they dissolve in cold nitric acid, forming a deep red solution, similar to that of morphia in nitric acid ; heat destroys this colour instantly, and the solution, after boiling, on being diluted with water, throws down a white flaky precipitate. The crystals are insoluble in muriatic and in acetic acids, and in the caustic alkalies, hot or cold.

The oil is soluble in ether and alcohol, but insoluble in water ; distilled with water, it passes over like the greater number of the essential oils, without having undergone any alteration ; but when a portion was attempted to be distilled alone, the greater part came over in the form of a thick oily matter, a considerable quantity of sulphurous acid was formed, and charcoal and a little sulphuric acid were left in the retort. With a view to get rid of a portion of acid, which the carbonate of potash had apparently not removed, some of the oil was heated in a solution of caustic potash ; it diminished considerably in bulk, and became much more viscid than before : it was separated from the potash solution by the action of ether, and when the ether was distilled off, there remained a yellow oil, with very little fluidity, which evaporated entirely when heated, without any appearance of decomposition or evolution of sulphurous acid, and which, in a few days, concreted into a mass of prismatic crystals, having all the characters of those before described. The potash solution evaporated to dryness, afforded a residue somewhat like acetate of potassa in appearance ; upon heating a few grains of it, it took fire, and burnt with a flame resembling that of alcohol, and sulphate of potash remained ; it dissolved in hot alcohol, and the solution deposited, on cooling, crystals in the form of pearly scales ; in short, it had those characteristics which have been ascribed to sulphovinate of potassa ; I therefore consider *oil of wine* as a compound of sulphovinic acid, and the peculiar crystallizable oil which I have described.

There are two facts which render it probable that oil of wine, when obtained, as in the above process, from alcohol and sulphuric acid, is a product of the decomposition of sulphovinic acid; namely—first, that when alcohol and sulphuric acid are mixed in equal bulks, sulphovinic acid is formed in great abundance; nearly five ounces of sulphate of lead were obtained from the sulphovinate of that metal, formed by neutralizing the acid resulting from a mixture of four ounces of alcohol with an equal bulk of sulphuric acid, the mixture having been allowed to become cold before it was saturated—and secondly, oil of wine, or a fluid exactly resembling it, is obtained when any of the sulphovinates are carefully decomposed by heat.

Apothecaries' Hall, March 15, 1825.

ART. XI.—Proceedings of the Royal Society of London.

Thursday, Dec. 16, 1824.—At this meeting, Sir Charles Wetherell, Knt., and John Bell, Esq. were elected Fellows of the Society, and a paper was communicated by Dr. Roget, entitled an *Explanation of an Optical Deception in the appearance in the spokes of a wheel, seen through vertical apertures.*

A paper was also read containing a description of a *New Photometer*, by Mr. Ritchie.

Thursday, Dec. 23.—Captain F. W. Beechy was elected a Fellow of the Society.

Two supplementary papers to Mr. Powell's former communication, upon *Radiant Heat*, were read, and the Society then adjourned over the Christmas Vacation.

Thursday, January 13, 1825.—Mr. William Scoresby was admitted into the Society.

A paper was communicated by Captain Henry Kater, being a description of a *Floating Collimator*.

January 20.—Captain James Mangles, R. N., was elected a Fellow of the Society.

A paper on the *Construction of the Barometer* was communicated by John Frederic Daniell, Esq.

January 27.—The Rev. George Fisher was elected a Fellow of the Society.

Part of a paper on the *Anatomy of the Mole Cricket*, by Dr. John Kidd, was read.

February 3.—Viscount Strangford was elected a Fellow of the Society, and the reading of Dr. Kidd's paper was resumed and concluded.

Sir Everard Home gave in a postscript to his Croonian Lecture, in which he announced the discovery of nerves in the human placenta and umbilical chord.

February 10.—A paper on the *Iguanodon*, by Gideon Mantell, Esq. was read.

February 17.—Mr. Henry Harvey was elected a Fellow of the Society.

A paper was communicated by the Rev. Baden Powell, entitled *An Experimental Inquiry into the radiant heating effects from Terrestrial Sources*.

February 24.—Dr. John Richardson and Joseph Henry Green, Esq. were elected Fellows of the Society,

Part of a paper on the *Maternal Fætal Circulation*, by Dr. Williams, was read.

March 3.—Dr. Lewis Tiarks was elected a Fellow of the Society, and the reading of Dr. Williams's paper was resumed and concluded.

A paper was read, containing *Further Observations on the Planariæ*, by Dr. I. R. Johnson.

March 10.—Major-General Sir William D'Urban was elected a Fellow of the Society.

A paper by Mr. W. Ritchie was read, entitled *Improvements on Leslie's Photometer.*

March 17.—A paper was communicated by Sir Everard Home, on the *Influence of Nerves and Ganglions in producing Animal Heat.*

ART. XII. ANALYSIS OF SCIENTIFIC BOOKS.

I. *Philosophical Transactions of the Royal Society of London; for the year 1824. Part II.*

THE first paper in this part of the Transactions is entitled,

Some curious Facts respecting the Walrus and Seal, discovered by the examination of Specimens brought to England by the different ships lately returned from the Polar Circle. By Sir Everard Home, Bart. V.P.R.S. In a letter addressed to Sir Humphry Davy, Bart. P.R.S.

The first part of these "curious facts" is a peculiarity in the hind foot of the Walrus, which enables it to carry on progressive motion "against gravity;" this is effected by an apparatus resembling that of the foot of the fly, and in operation, not unlike a cupping glass: the anatomical details are illustrated by two plates; they exhibit, as far as bony structure is concerned, a striking resemblance to the human hand.

The second of Sir Everard's discoveries, is the mode in which the bile in the Walrus is collected in a reservoir, and thence impelled with great force into the duodenum. The anatomical structure, as shown in the plate annexed to this paper, is very peculiar.

Sir Everard's third new fact, is the peculiar structure of the funis and placenta of the seal; the trunks forming the funis are not twisted together; "their whole length is nine inches; three inches from the placenta they begin to give off branches, which freely anastomose with one another; these branches are connected to the placenta itself by three membranous folds, like so many mesenteries; between these folds the blood-vessels are conveyed to the substance of the placenta, on the surface of which they ramify to a great degree of minuteness. This structure will give a greater facility than common to the circulation through the placenta, which makes it an object of inquiry, whether the same peculiarities exist in other marine animals."

In another communication, printed in this part of the Transactions, Sir Everard describes the organs of generation of the Mexican Proteus, but the details of this paper require the plates to render them intelligible; they are engraved from Mr. Bauer's exquisite drawings, and display some singular physiological provisions.

Additional Experiments, and Observations, on the application of Electrical Combinations to the preservation of the Copper Sheathing of Ships, and to other purposes. By Sir H. Davy, Bart. P.R.S.

AN abstract of this paper will be found at page 279 of our seventeenth volume.

On the Apparent Direction of Eyes in a Portrait. By W. H. Wollaston, M.D., V.P.R.S.

OF this paper we have also already given some account. (Vol. XVII. p. 274.) It is accompanied by several well-devised engravings, which might, we think, furnish the basis of a series of very amusing and instructive illustrations of the rules of perspective.

Further Particulars of a Case of Pneumato-thorax. By J. Davy, M.D., F.R.S.

THE details of this paper will be found in one of our preceding numbers*.

On the Action of finely-divided Platinum on Gaseous Mixtures, and its Application to their Analysis. By William Henry, M.D., F.R.S.

THE curious discovery of Professor Doebereiner, of Jena, respecting the ignition of platinum, when in a spongy form, on the contact of a mixture of oxygen and hydrogen, is well known to our chemical readers; and we have detailed, from the *Annales de Chimie*, several interesting facts respecting it, ascertained by MM. Dulong and Thenard. To these Dr. Henry has made some important additions.

The first section of his paper relates to the action of finely-divided platinum on gaseous mixtures, at common temperatures, and the second to its effect at increased temperatures; of these results an abstract has already been given in a former number of this Journal†.

* No. XXXIV. page 263.

† No. XXXIV. page 277.

In the third section, he applies the facts detailed in the former two, to the analysis of mixtures of combustible gases in unknown proportions. For this purpose, he caused a quantity of gas to be collected from coal, by continuing the application of heat to the retorts two hours beyond the usual period, and receiving the gas into a separate vessel. Gas of this quality was purposely chosen, because, from former experience, it was expected to contain free hydrogen, carbonic oxide, and carburetted hydrogen, but no olefiant gas, the production of which is confined to the early stages of the progress. After washing it, therefore, with liquid potash, to remove a little carbonic acid, and ascertaining its specific gravity when thus washed to be .308, he proceeded at once to subject it to the following method of analysis.

Having ascertained, by a previous experiment with Volta's eudiometer, that ten volumes of the gas required for saturation 9 volumes of oxygen, he mixed 43 measures with 43 of oxygen (= 41 pure) and passed a platinum ball, which had been recently heated, into the mixture. An immediate diminution of volume took place, attended with a production of heat, and formation of moisture. The residuary gas, cooled to the temperature of the atmosphere, measured 43.5 volumes. Of these, 4.5 were absorbed by liquid potash, indicating 4.5 carbonic acid, equivalent to 4.5 carbonic oxide; the rest, being fired in a Volta's eudiometer with an additional quantity of oxygen, gave 11 volumes of carbonic acid; the diminution being 22 volumes, and the oxygen consumed 22 also, circumstances which prove that 11 volumes of carburetted hydrogen were consumed by this rapid combustion. But of the loss of volume first observed, (viz. $86 - 43.5 = 42.5$) 2.25 are due to the carbonic acid formed; and deducting this from 42.5, we have 40.25, which are due to the oxygen and hydrogen converted into water; and $40.25 \times \frac{2}{3} = 26.8$ shows the hydrogen in the original gas. But the sum of these numbers ($26.8 + 4.5 + 11$) being less by 0.7 than the volume of gas submitted to analysis, we may safely consider that fraction of a measure to have been nitrogen. The composition then of the mixture will stand in volumes as follows :

Hydrogen	26.8	. . .	62.32
Carbonic oxide	4.5	. . .	10.50
Carburetted hydrogen	11.0	. . .	25.56
Nitrogen	0.7	. . .	1.62
	43.0		100. 0

On calculating what should be the specific gravity of a mixture of gases in the above proportions, it was found to be .303*, which

* In this estimate, the specific gravity of hydrogen is taken at .0694; that of carbonic oxide at .6722; of carburetted hydrogen at .5555; and of nitrogen at .9728.

which coincides, as nearly as can be expected, with the actual specific gravity of the gas submitted to analysis, viz. .308. To place the correctness of the results beyond question, our author mingled the gases in the above proportions, and acted on the artificial mixture in the same manner as on the original gas, when he had the satisfaction to find that the analytical process again gave the true volumes with the most perfect correctness for the hydrogen and carbonic oxide, and within the fraction of a measure for the carburetted hydrogen. Notwithstanding this successful result, which was twice obtained, Dr. Henry observes that he should still prefer, for the reason which has been stated, having recourse to a temperature carefully regulated, for the analysis of similar mixtures, in all cases where the hydrogen is in moderate proportion, and where great accuracy is desirable. Whenever (it may again be remarked) olefiant gas is present in a mixture, it should always be removed by chlorine, before proceeding to expose the mixture to the agency of the spongy metal.

It can scarcely be necessary to enter into further details respecting methods of analysis, the application of which to particular cases must be sufficiently obvious, from the experiments which have been described on artificial mixtures. The apparatus required is extremely simple, consisting, when the balls are employed, of graduated tubes of a diameter between 0.3 and 0.6 of an inch; or, when an increased temperature is used, of tubes bent into the shape of retorts, of a diameter varying with the quantity of gas to be submitted to experiment, which may be from half a cubic inch to a cubic inch or more. These, when in use, may be immersed in a small iron cistern containing mercury, and provided with a cover in which are two holes, one for the tube, and the other for the stem of a thermometer, the degrees of which are best engraved on the glass.

"By means of these improved modes of analysis," says our author, "I have already obtained some interesting illustrations of the nature of the gases from coal and from oil. I reserve, however, the communication of them, till I have had an opportunity of pursuing the inquiry to a greater extent, and especially of satisfying myself respecting the exact nature of the compound of charcoal and hydrogen, discovered some years ago by Mr. Dalton, in oil gas, and coal gas, which agrees with olefiant gas in being condensable by chlorine, but differs from it in affording more carbonic acid and consuming more oxygen."

We have anxiously looked for Dr. Henry's further remarks upon the very interesting subject of this communication, more especially as relating to the constitution of oil gas, connected with which there are so many curious and anomalous circumstances, that we are convinced their thorough investigation would

throw much new light upon the nature of the compounds of carbon and hydrogen, respecting which very much remains yet to be ascertained.

A Comparison of Barometrical Measurement with the Trigonometrical Determination of a Height at Spitzbergen, by Captain Edward Sabine, F.R.S.

OF this paper we have already given an abstract,* and also of the following one, containing *Experimental Inquiries relative to the distribution and changes of the Magnetic Intensity in ships of war, by George Harvey, Esq.*†

Experiments on the Elasticity and Strength of hard and soft Steel, by Mr. Thomas Tredgold.

IF a piece of very hard steel be softened, it is natural to suppose that the operation will produce a corresponding change in the elastic power, and that the same load would produce a greater flexure in the soft state than in the hard one, when all other circumstances were the same. Mr. Coulomb inferred from some comparative experiments on small specimens, that the state of temper does not alter the elastic force of steel; and Dr. Young's Experiments on Vibration led to the same conclusion (Nat. Philos. II. 403). But the subject appeared to require further investigation, and particularly because it afforded an opportunity of ascertaining some other facts respecting steel, which had not been before examined.

In making the experiments described in this paper, each bar was supported at its ends by two blocks of cast iron. These blocks rested upon a strong wooden frame. The scale to contain the weights was suspended from the middle of the length of the bar, by a cylindrical steel pin of about three-eighths of an inch in diameter. And as in experiments of this kind it is desirable to have the means of raising the weight from the bar, without altering its position, in order to know when the load is sufficient to produce a permanent change of structure, a powerful screw with a fine thread was fixed over the centre of the apparatus, by which the scale could be raised or lowered, when the cords on which the screw acted were looped on to the cross pin by which the scale was suspended.

To measure the flexure, a quadrantal piece of mahogany was fixed to the wooden frame; two guides were fixed on one edge of the mahogany, in which a vertical bar slid, and gave motion to an index. The bar and index were so balanced, that one

* Vol. XVII. p. 268.

† Ibid. p. 261.

end of the bar bore with a constant pressure on the specimen, and the graduated arc over which the index moved was divided into inches, tenths, and hundredths; and thousands were measured by a vernier scale on the end of the index. By a screw at the lower end of the vertical bar, the index was set to zero, when necessary.

The first trials were made with a bar of blistered steel of a very good quality. It was drawn out by the hammer to a proper width and thickness, and then filed true and regular. It was then hardened, and tempered to the same degree of hardness as common files.

The total length of the bar was 14 inches; the distance between the supports 13 inches; the breadth of the bar 0.95 inches, and the depth 0.375 inches: the thermometer varied from 55° to 57° at the times of trial.

With a load of 54 lbs. the depression in the middle was 0.02 in.

82	-	-	-	-	-	-	0.03	„
110	-	-	-	-	-	-	0.04	„

The last load remained on the bar some hours, but produced no permanent alteration of form.

The temper of the bar was then lowered to a rather deep straw yellow, and it was tried again; when the same loads produced exactly the same flexures as before.

The temper was then lowered till the colour was an uniform blue, or spring temper; and the trials were repeated with the same loads; but the flexures were still the same.

It was now heated to redness and very slowly cooled. In this state the same loads still produced the same flexures; and the load of 110 lbs. caused no permanent change of form.

The bar was hardened again, and made very hard; in this state the same loads produced the same flexures; and

With a load of 300 lbs. the depression in the middle was 0.115 in.

350	-	-	-	-	-	-	0.130	„
580	-	-	-	-	-	-	broke.	

When the bar was relieved from the load of 350 lbs. it retained a permanent flexure of 0.005 inches, which increased to 0.01 with the addition of 10 lbs. to the load.

Finding that a bar of much greater length might be tempered without difficulty, Mr. Tredgold had another bar made of the same kind of steel; the length of which being 25 inches, about double the flexure could be given with the same strain upon the material; and therefore any small degree of difference in the elastic force might be more easily detected, for the preceding experiments showed that if there be any difference, it must be extremely small.

The breadth of this bar was 0.92 inches; the depth 0.36

inches; and the distance between the supports 24 inches. It was soft, so as to yield easily to the file.

With a load of 18.6 lbs. the depression in the middle was 0.05 in.

37.0	-	-	-	-	-	-	0.10	„
47.0	-	-	-	-	-	-	0.127	„

The bar was then hardened, so that a file made no impression on any part of it, and the same loads did not produce flexures that were sensibly different from those in the soft state.

The temper was then lowered till it assumed an uniform straw colour; when with a load of

47lbs. the depression in the middle was 0.127 inches.

85	.	.	.	.	.	.	.	.	0.230	„
130	.	.	.	.	.	.	.	.	0.350	„
150	.	.	.	.	.	.	.	.	0.400	„

The load of 150lbs. produced a permanent set of 0.012, but 130lbs. produced no sensible effect. The loading was continued, and with

185lbs.	the depression in the middle was	0.50	inches,
385		1.04	„

When 385lbs. had been upon the bar about a minute, it emitted a faint creaking sound, and consequently no more weight was added; in about fourteen minutes the bar broke, exactly in the middle of the length.

On comparing the fractures of the specimens, there was no apparent difference except in colour. The grain was fine, and equal; the small sparkles of metallic lustre abundant, and equally diffused; but in the harder specimen they had a whiter ground.

From these experiments it appears that the elastic force of steel is sensibly the same in all states of temper.

The height of the modulus of elasticity, calculated by the formula given by Dr. Young in his *Nat. Phil.* (Vol. II. p. 48,) is, according to the first experiment, . . . 8,827,300 feet. And according to the second experiment . . . 8,810,000 feet.

Now the height of the modulus, as has been determined by Dr. Young for steel by experiments on vibration, is 8,530,000 feet. (*Nat. Phil.* II. p. 86.) The modulus for cast steel calculated from Duleau's experiments (*Essai Théorique et Expérimental sur le Fer Forgé*, p. 38,) is 9,400,000 feet, and for German steel 6,600,000 feet.

The force which produces permanent alteration is, to that which causes fracture in hard steel, as 350 : 580; or as 1 : 1.66; in the same steel of a straw-yellow temper as 150 : 385, or as 1 : 2.56.

When the tension of the superficial particles at the strain which causes permanent alteration, is calculated by the formula given in Mr. Tredgold's *Essay on the Strength of Iron*, p. 146, Second Edition, it is 45,000lbs. upon a square inch in tempered steel; and the absolute cohesion 115,000lbs. Mr. Rennie found the direct

cohesion of blistered steel to be 133,000lbs. (*Philosophical Transactions* for 1818.)

But in the very hard bar, the strain which produced permanent alteration was 51,000lbs. for a square inch, and the absolute cohesion only 85,000lbs.

From these comparisons I think it will appear, that in the hardening of steel, the particles are put in a state of tension among themselves, which lessens their power to resist extraneous force. The amount of this tension should be equal to the difference between the absolute cohesion in different states. Taking Mr. Rennie's experiment as the measure of cohesion in the soft state, it will be $133,000 - 115,000 = 18,000$ lbs. for the tension with a straw-yellow temper; and $133,000 - 85,000 = 48,000$ lbs. for the tension in hard steel. And if this view of the subject be correct, the phenomena of hardening may be explained in this manner, which nearly agrees with what Dr. Young has observed in his *Lecture I*, p. 644: after a piece of steel has been raised to a proper temperature, a cooling fluid is applied capable of abstracting heat more rapidly from the surface than it can be supplied from the internal parts of the steel. Whence the contraction of the superficial parts round the central ones which are expanded by heat; and the contraction of the central parts in cooling, while they are extended into a larger space than they require at a lower temperature, produces that uniform state of tension, which diminishes so much the cohesive force in hard steel. The increase of bulk by hardening agrees with this explanation; and it leads one to expect, that any other metal might be hardened if we could find a means of abstracting heat with greater velocity than its conducting power.

A short Account of some Observations made with Chronometers, in two Expeditions sent out by the Admiralty, at the recommendation of the Board of Longitude, for ascertaining the Longitude of Madeira and of Falmouth. In a Letter to Thomas Young, M.D., F.S.R.S., and Secretary to the Board of Longitude. By Dr. John Lewis Tiarks.

THE results of these observations are given at p. 270, Vol. XVII.

Of the Effects of the Density of Air on the Rates of Chronometers, by George Harvey, Esq.

WE have elsewhere shortly noticed the contents of this paper*, which occupies forty of the quarto pages of the *Philosophical Transactions*; we must therefore refer those who are interested in the minute details of such an inquiry to the original document.

* Vol. XVII. p. 272.

A Letter from L. W. Dillwyn, Esq. to Sir H. Davy, Bart., P.R.S.

(See an abstract of this Letter, Vol. XVII. p. 267.)

An Account of Experiments on the Velocity of Sound, made in Holland, by Dr. G. Moll and Dr. A. Van Beek.

Two and thirty pages of tabular details contain the researches of these learned Dutchmen, on the above subject; these we shall neither insert nor abridge, but rest content with laying the following table before our readers, which shows the result of Experiments on the Velocity of Sound, as observed by different philosophers, reminding them that the French metre is equal to 39.37 English inches.

Names of Observers.	Time when made.	Country where made.	Length of basis. Metres.	Velocity of Sound per Second in metres.	
Mersenne		France		448	1
Florentine Philosophers	1660	Italy	1800	361	2
Walker	1698	England	800	398	3
Cassini, Huigens, &c.		France	2105	351	4
Flamstead and Halley		England	5000	348	5
Derham	1704 and 1705	England	1600 à 2000	348	6
French Academicians	1738	France	22913 and 28526	332,93 at 0° c	7
Blanconi	1740	Italy	24000	318	8
La Condamine	1740	Quito	20543	339	9
La Condamine	1744	Cayenne	39429	358	10
T. F. Mayer	1778	Germany	1040	336,86	11
G. E. Muller	1791	Germany	2600	338	12
Epinoza and Banza	1794	Chili	16345	356,14 at 0°	13
Benzenberg	1809	Germany	9072	333,07 at 0°	14
Arago, Mathieu Prony	1822	France	18612	331,05 at 0°	15
Moll, Van Beck, and Kuytenbrouwer.	1823	Netherlands	17669,28	{ 332,05 at 0° } { and dry air. }	16

1. Mersenne de Aste Ballistica Prop. 39.
2. Tentamina Experim. Acad. del Cimento, L. B, 1738, Part II. p. 116.
3. Philos. Trans. 1698, No. 247.
4. Duhamel, Hist. Acad. Regi. L. II. Sect. 3, Cap. II.
5. Philos. Trans. 1708 and 1709.
6. Ibid, ibid.
7. Mem. de l'Academie des Sciences, 1738 and 1739.
8. Comment. Bononienses, Vol. II. p. 365.
9. La Condamine Introduction Historique, &c. 1751, p. 98.
10. Mem. de l'Acad. Royale des Sciences, 1745, p. 488.
11. J. T. Mayer, Praktische Géométrie Gottingen, 1792, B. 1. p. 166.
12. Muller, Gotting. Gelehrt. Anzeige, 1791, St. 159. and Voigt's Magazin, &c. B. 8, St. 1. p. 170.
13. Annales de Chimie et de Phys. T. VII. p. 93.
14. Gilbert's Annalen, neue Folge, B. V. p. 383.
15. Connaissance des Tems. 1825, p. 361.

A Catalogue of nearly all the principal Fixed Stars, between the Zenith of Cape Town, Cape of Good Hope, and the South Pole, reduced to the 1st of January, 1824. By the Rev. F. Fallows.

Remarks on the Parallax of α Lyræ, by J. Brinkley, D.D.

Of this, which is the concluding paper of the volume before us, we have already given a pretty copious abstract. [Vol. XVII. p. 264.]

ART. XIII. ASTRONOMICAL AND NAUTICAL
COLLECTIONS.

No. XXI.

i. *Remarks on the determination of the LONGITUDE, from Observations of the Moon's Right Ascension.* By THOMAS HENDERSON, Esq.

IN the *Connaissance des Temps*, for 1825, p. 345, M. Bouvard gives a formula for the computation of the difference of longitude between two places, by means of the observed motion of the moon in right ascension, during the interval elapsed in passing from the one meridian to the other. It is this,

$$d = ar \left(\frac{15^\circ + m - h}{h} \right)$$

where a represents the observed motion of the moon in right ascension expressed in sidereal time, r the ratio of sidereal to mean time, m the mean horary motion of the sun, h the horary motion of the moon in right ascension for one hour mean time, and d the difference of longitude required.

It appears to me that this formula is erroneous, in so far as r is introduced; and that it ought to have been

$$d = a \left(\frac{15^\circ + m - h}{h} \right)$$

Which may be demonstrated thus:—

From the observed right ascension of the moon upon the meridian of the first place, calculate the mean time at Greenwich of the observation, and the right ascension of the meridian at Greenwich, by adding to the mean time there the sun's mean longitude at this time. The difference between the right ascension of the meridian at Greenwich, and the right ascension of the meridian at the place of observation, (being the same as the observed right

ascension of the moon,) is evidently the longitude of the first place from Greenwich.

In like manner let the longitude of the second place from Greenwich be calculated, and the difference between the two longitudes will be the difference of longitude required.

Comparing the two calculations, and employing the same notation as before, the difference between the mean times at Greenwich of the respective observations is =

$$\frac{a \times 15^\circ}{h}$$

The difference between the sun's mean longitudes at these times is = $\frac{am}{h}$.

And hence the difference between the right ascensions of the meridian at Greenwich at the respective times is =

$$a \left(\frac{15^\circ + m}{h} \right)$$

And the difference of longitude between the two places is =

$$a \left(\frac{15^\circ + m}{h} \right) - a = a \left(\frac{15^\circ + m - h}{h} \right)$$

An example will make this quite clear.

Let the moon's right ascension upon the meridian of the first place be $0^h 0' 0''$, and the sun's mean longitude the same. The moon will pass the meridian of the first place at $0^h 0' 0''$ mean time.

Suppose that the daily increase of the moon's right ascension is $1^h 0' 0''$, and of the sun's mean longitude $0^h 4' 0''$. Then the second place being supposed to be in $12^h 0' 0''$ of the west longitude from the first, the time of the moon passing the meridian of the second is thus found, by the rule given in the explanation of the *Nautical Almanac*.

At mean noon, under the meridian of the second place, the moon's right ascension is $0^h 30' 0''$, and the sun's mean longitude is $0^h 2' 0''$, the difference between which is $0^h 28' 0''$. Then as $24^h + 0^h 4' 0'' - 1^h 0' 0'' = 23^h 4' 0'' : 0^h 28' 0'' :: 1^h 0' 0'' - 0^h 4' 0'' = 0^h 56' 0'' : 0^h 1' 8''$. And $0^h 28' 0'' + 0^h 1' 8'' =$

0^h 29' 8" mean time of the moon passing meridian of second place ; at which time the sun's mean longitude will be found to be 0^h 2' 4.8", the moon's right ascension 0^h 31' 12".8, the difference between which 0^h 29' 8" is the mean time of the moon's passing the meridian already found, and therefore confirms the calculation.

The increase of the moon's right ascension in passing between the two meridians is 31' 12".8, from which M. Bouvard's formula makes the difference of longitude 11^h 58' 0", being 2' 0" erroneous, as the annexed calculation shows.

$$\text{Log. of } a, 31' 12".8 \dots\dots\dots 3.2724914$$

$$\text{Log. of } r, \frac{24^h}{24^h 4'} \dots\dots\dots 9.9987953$$

$$\begin{array}{r} 15^\circ 0' 0'' \\ + m \quad + 2 \quad 30 \\ - h \quad - 37 \quad 30 \\ \hline \end{array}$$

$$\text{Log of } \dots\dots\dots 14 \quad 25 \quad 0 \dots\dots\dots 4.7151674$$

$$\text{A. C. Log. of } h \dots\dots\dots 0 \quad 37 \quad 30 \dots\dots\dots 6.6478175$$

$$\text{Log. of } d \dots\dots\dots 11^h 58 \quad 0 \dots\dots\dots 4.6342716$$

The corrected formula is the same as M. Bouvard's, with the exception of r being expunged. It consequently makes the logarithm of d 4.6354763, and $d = 11^h 59' 59".3$, which is as accurate as the data will admit of. The same result is obtained from the rules given by Professors Vince and Woodhouse, in their Treatises on Astronomy.

It therefore follows that M. Bouvard's calculations of the difference of longitude between Greenwich and Paris must be corrected, by adding to each result 1".53, the amount of the error occasioned by his introducing r into the calculations.

It should be attended to, that different observers and different instruments do not always give the same measurements of the moon's diameter, and consequently that the mean longitude deduced from a number of observations of one limb of the moon, may differ from that deduced from observations of the other. The mean result of the whole observations, taken together, will be somewhat affected by the error arising from this cause, unless an equal number of observations of each limb has been made. When this has

not been done, the average result from the observations of each limb should be taken, and the mean of these held as the true longitude deduced from the whole observations.

Upon these principles, M. Bouvard's calculations of the difference of longitude between Greenwich and Paris will stand thus:—

From 31st August, 1800, to 6th August, 1803, when the observations both at Greenwich and Paris were made with the old transit instruments, those at Greenwich being by Dr. Maskelyne, and his assistants;

Mean of observations of first limb . .	9' 24"·75
Mean of observations of second limb	9 20 ·24
Mean of both	9' 22"·50

From 7th September, 1803, to 11th April, 1811, when the observations at Greenwich were made with the old transit instrument by Dr. Maskelyne, and his assistants, and those at Paris with the new transit instrument;

Mean of first limb	9' 25"·85
Mean of second limb	9 18 ·07
Mean of both	9' 21"·96

From 14th May, 1811, to 2d June, 1816, when the observations at Greenwich were made with the old transit instrument by Mr. Pond, and his assistants, and those at Paris with the new transit instrument;

Mean of first limb	9' 20"·10
Mean of second limb	9 20 ·36
Mean of both	9' 20"·23

From 5th July, 1816, to 1st December, 1819, when the observations at Greenwich were made with the new transit instrument, and those at Paris, as formerly;

Mean of first limb	9' 21"·43
Mean of second limb	9 20 ·50
Mean of both	9' 20"·96

The formula $d = a \left(\frac{15^\circ + m - h}{h} \right)$ may be expressed in another manner, which will be found more convenient for calculation,

Let $R = \frac{15^\circ + m}{15}$ = be the ratio of mean time to sidereal, the logarithm of which is 0.0011873; then by eliminating the value of m from this expression, and substituting it, we obtain

$$d = aR \frac{15^\circ}{h} - a.$$

The multiplier $\frac{15^\circ}{h}$ expresses the ratio of one hour to the moon's motion in right ascension during that period, which is evidently the same as the ratio of any given period of time to the moon's motion in right ascension during the same period. Now if the moon's motion in right ascension during twelve hours in degrees, minutes, and seconds, be depressed a denomination, and reduced to minutes, seconds, and thirds, this will be the moon's motion in right ascension in time for three hours, the logarithm of the ratio of the latter of which to the former, and consequently of $\frac{15^\circ}{h}$ is equal to the proportional logarithm of the motion in time for three hours. And thus the logarithm of $\frac{15^\circ}{h}$ is obtained by inspection.

The above formula $d = aR \frac{15^\circ}{h} - a$, is adapted to mean time, but it will equally answer for apparent time, provided R be held to express the ratio of apparent time to sidereal, and the logarithm of $\frac{15^\circ}{h}$ be taken equal to the proportional logarithm of the moon's motion in right ascension in time, during three hours apparent time. The value of R will vary with the sun's motion in right ascension. A table of the logarithms of R is subjoined, in which the argument is the sun's motion in right ascension in time for twenty four hours apparent time. This manner of applying the formula is more convenient for the use of the *Nautical Almanac*.

The moon's motion in right ascension is always supposed to be that answering to the middle point of time between her passages over the respective meridians.

The following is an example of the last modification of the formula.

M. Bouvard makes the increase of the moon's right ascension, during her passage from the meridian of Paris to that of Greenwich, on 17th January, 1818, to be $20''\cdot07$ in time. Now the increase of the sun's right ascension in twenty four hours apparent time at that time was $4' 16''\cdot1$; the middle point of time between the moon's passages over the respective meridians was $8^h 22'$ apparent time at Paris, when the moon's motion in right ascension for twelve hours apparent time was (by the *Connaissance des Temps*.) $6^o 13' 44''$, which depressed a denomination becomes $6' 13'' 44'''$, the moon's motion in right ascension in time for three hours.

Log. of $20''\cdot07$		1.30255
Log. of R (argument $4' 16''\cdot1$)		0.00128
Prop. log. of $6' 13'' 44'''$		1.46085
Log. of $9' 41''\cdot6$		2.76468
Subtract	$20\cdot07$	
Remains longitude	$9 21\cdot5$	

M. Bouvard makes the longitude $9' 20''\cdot0$ from the observation; but the correction $1''\cdot5$ being applied, it becomes $9' 21''\cdot5$, as above.

TABLE OF THE LOGARITHMS OF THE RATES OF APPARENT TIME TO SIDEREAL.

Argument.—Sun's motion in Right Ascension in time for 24 hours apparent time.

Arg.	Log.	Arg.	Log.	Arg.	Log.
3' 30"	0.00105	3' 40"	0.00110	3' 50"	0.00115
3 31	0.00106	3 41	0.00111	3 51	0.00116
3 32	0.00106	3 42	0.00111	3 52	0.00116
3 33	0.00107	3 43	0.00112	3 53	0.00117
3 34	0.00107	3 44	0.00112	3 54	0.00117
3 35	0.00108	3 45	0.00113	3 55	0.00118
3 36	0.00108	3 46	0.00113	3 56	0.00118
3 37	0.00109	3 47	0.00114	3 57	0.00119
3 38	0.00109	3 48	0.00114	3 58	0.00119
3 39	0.00110	3 49	0.00115	3 59	0.00120

Arg.	Log.	Arg.	Log.	Arg.	Log.
4' 0"	0.00120	4 10"	0.00125	4' 20"	0.00130
4 1	0.00121	4 11	0.00126	4 21	0.00131
4 2	0.00121	4 12	0.00126	4 22	0.00131
4 3	0.00122	4 13	0.00127	4 23	0.00132
4 4	0.00122	4 14	0.00127	4 24	0.00132
4 5	0.00123	4 15	0.00128	4 25	0.00133
4 6	0.00123	4 16	0.00128	4 26	0.00134
4 7	0.00124	4 17	0.00129	4 27	0.00134
4 8	0.00124	4 18	0.00129	4 28	0.00135
4 9	0.00125	4 19	0.00130	4 29	0.00135

The following is an example of a calculation of the longitude from an observed right ascension of the moon upon the meridian, compared with the ephemeris, by the rule given in the beginning of this paper.

On 15th April, 1818, the right ascension of the moon's first limb, when upon the meridian of Greenwich, was observed =

	142° 11' 48"
Semidiameter in right ascension . .	16 32
Right ascension of moon's centre . .	142 28 20

From the *Connaissance des Tems*, we have D. A. R.

1818

April 14, Midn. 131 35 17

14, Midn.	131	35	17			
				6 ⁰	31 ¹	54 ²
15, Noon	138	7	11			4 ¹ 45 ²
				6	27	9
Midn.	144	34	20			4 ¹ 38 ²
				6	22	37

16, Noon 150 56 57

142 28 20

138 7 11

$$\text{As. } 6^{\circ} 27' 9'' : \overline{4 \ 21 \ 9} :: 12^{\text{h}} 0' 0''$$

A. C. Log. 6 27 9 = 5 633970

$$\text{Log. } 4219 = 4.195041$$
$$\text{Log. } 12 \quad 0 \quad 0 = 4.635484$$
$$\overline{4.464495} = \text{Log. of}$$

Approximate time at Paris $8^h 5' 40''$

Equation of second difference $+ 30'' \cdot 5$ in time = 57

Apparent time Paris = 8 4 43

Sun's right ascension then = 1 33 29.7

Right ascension of meridian at Paris = $\overline{9\ 38\ 127}$

Observed R. A. of mer. at Greenwich $142^{\circ} 11' 48'' = 9\ 28\ 47.2$

Longitude 9 25.5

ii. *Discordance of the Lunar Observations made at GREENWICH, and at PARIS. In a Letter from THOMAS HENDERSON, Esq.*
SIR,

In making some calculations from the observations made at the Royal Observatories of Greenwich and Paris, I have been puzzled by a circumstance, which I do not observe has been yet taken notice of, but which seems to be highly deserving the attention of Astronomers. It is this. From 1800 to 1809, the observations made with the transit instrument at Greenwich show the sun and moon to be further advanced in right ascension, than those made with the transit instrument at Paris. From a mean of a great many observations, the difference appears to be about 3 or 4 seconds of space. From the Introduction to Burg's Lunar Tables, I observe that the epoch of the moon's mean longitude for 1801 is $3\frac{1}{2}$ seconds greater by the Greenwich observations, than by those at Paris.

I find a similar difference, upon comparing the observations of the sun and moon's right ascensions, made with the transit instrument and mural circle at Greenwich, in the year 1812, the right ascension of these bodies by the transit instrument being about 4 seconds of space less than by the circle.

This discrepancy appears to be occasioned by some difference in the methods of observing the transits of the sun and moon, and of the stars, by the respective instruments. Should its cause be found out and obviated, the accuracy of the observations of the sun and moon would evidently be much improved. To accomplish this, the fittest course appears to be, to call the attention of astronomers to the fact, through the medium of your valuable *Astronomical and Nautical Collections*.

It is owing to this circumstance, that the difference of longitude between Greenwich and Paris, deduced from the moon's meridian right ascensions, is greater than the true difference. See my paper on this subject which I formerly transmitted to you.

I am, very respectfully,

Sir,

Edinburgh,
31st Dec. 1824.

Your very obedient humble servant,
THOMAS HENDERSON.

iii. A Rule for clearing the LUNAR DISTANCE from the effects of Parallax and Refraction. By CHARLES BLACKBURNE, Esq.

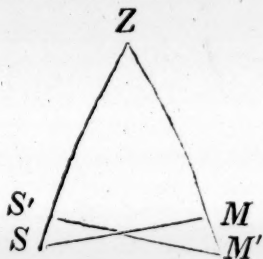
Let S and S' denote the true and apparent altitudes of $\odot$ or $\star$, M and M' , the true and apparent altitudes of the D , d the difference between the true altitudes, d' the difference between the apparent altitudes, Δ the apparent distance of the centres. Then,

RULE,

Add together the five following logarithms, viz. $\text{l. sin. } \frac{1}{2} \Delta + d'$; the $\text{l. sin. } \frac{1}{2} \Delta - d'$; the $\text{l. cosin. } M$; the $\text{l. sec. } M'$; the constant logarithm 0.301150, and reject 30 from the index.*

To the natural number belonging to this logarithm, add the $\text{v. sin. } d$; the result will be the v. sin. of the true distance†.

DEMONSTRATION.



Let ZS , ZS' , be the true and apparent zenith distances of the sun or a star; ZM , ZM' , those of the moon; then SM will be the true, and $S'M'$ the apparent distance of their centres. And by the principles of trigonometry,

$$\sin.^2 \frac{1}{2} Z = \frac{R^2 \times \sin. \frac{1}{2} (ZS' - ZM' + S'M') \times \sin. \frac{1}{2} (ZS' - ZM' - S'M')}{\sin. ZS' \times \sin. ZM'}$$

$$\text{or } \sin.^2 \frac{1}{2} Z = \frac{R^2 \times \sin. \frac{1}{2} \Delta + d' \times \sin. \frac{1}{2} \Delta - d'}{\sin. S' \times \cos. M'}$$

Again,

$$\cos. SM = \cos. ZS - ZM - \sin. ZS \times \sin. ZM \times \sin.^2 \frac{1}{2} Z \times \frac{2}{R^3}$$

$$\text{or } \cos. SM = \cos d - \cos. S \times \cos. M \times \sin.^2 \frac{1}{2} Z \times \frac{2}{R^3}$$

* The constant logarithm 0.301150 must be corrected, if necessary, by Table X. or XI. in page 33 of the Requisite Tables.

† If the index to the logarithm be 9, find the natural number to as many places as the tables are calculated to; if it be 10, to one more; if it be 8, to one less; and so on.

The rule requires no distinction of cases.

and therefore by substituting in this equation the former value of $\sin.^2 \frac{1}{2} Z$, we get

$$\cos. SM = \cos. d - \frac{\sin. \frac{1}{2} \Delta + d' \times \sin. \frac{1}{2} \Delta - d' \times \cos. S \times \cos. M \times 2}{\cos. S' \times \cos. M' \times R}$$

$$\text{or } \cos. SM = \cos. d - \sin. \frac{1}{2} \Delta + d' \times \sin. \frac{1}{2} \Delta - d' \times \cos. M \times \sec. M' \times \frac{2 \cdot \cos. S}{R^3 \cdot \cos. S'}$$

$$\text{or } v. \sin. SM = v. \sin. d + \sin. \frac{1}{2} \Delta + d' \times \sin. \frac{1}{2} \Delta - d' \times \cos. M \times \sec. M' \times \frac{2 \cdot \cos. S}{R^3 \cdot \cos. S'}, \text{ which is the same as the Rule. } *$$

EXAMPLE.

Let the apparent distance of the moon from a star be $51^\circ 28' 35''$; the apparent altitude of the star $24^\circ 48'$; that of the moon $12^\circ 30'$; and the hor. parallax $56' 15''$. Required the true distance.

$$\begin{array}{r} \Delta = 51^\circ 28' 35'' \\ d' = 12^\circ 18' 0'' \\ \hline 63^\circ 46' 35'' \\ 39^\circ 10' 35'' \end{array}$$

$$1. \sin. \frac{1}{2} \Delta + d' = 31^\circ 53' 17.5'' = 9.722851$$

$$1. \sin. \frac{1}{2} \Delta - d' = 19^\circ 35' 17.5'' = 9.525379$$

$$1. \cosin. M = 13^\circ 20' 42'' = 9.988112$$

$$1. \secant M' = 12^\circ 30' 0'' = 0.010418$$

$$\text{the constant logarithm } 0.301149$$

$$N = 353109 \quad . \quad . \quad 9.547909$$

$$v. s. d = 19882$$

$$v. s. \quad . \quad . \quad 372991 \quad 51^\circ 10' 13'' = \text{true dist.}$$

iv. *Catalogue of the Stars in a Southern Constellation, denominated the COMET of ENCKE. By Mr. CHARLES RUMKER.*

The positions are deduced from Mr. Rumker's own observations. The elements of Aberration and Nutation are obtained according to the method of Zach, *Tables Nouvelles*.

* The logarithm 0.301150 is the logarithm of the constant quantity $2 \times \frac{\cos. S}{\cos. S'}$.

Name of Star.	AR. media pro initio 1823	Præc. in AR.	Dec. med. pro initio 1823	Præc. in Dec.	Aberratio		Nutatio	
					in AR. S. G. M.	in Dec. S. G. M.	in AR. S. G. M.	in Dec. S. G. M.
<i>a</i> Enckii Cometæ	92 46 33.4 3 observ.	52.024	17 50 12.92 4 observ.	-0.966	5 27 27 1.32783	9 8 36 0.28810	5 29 32 1.27504	2 27 58 0.98424
<i>b</i>	93 42 24.19 4 observ.	52.038	17 4 54.3 4 observ.	-1.291	5 26 36 1.32599	9 9 38 0.36039	5 29 25 1.27647	2 27 16 0.98421
<i>c</i>	94 34 8.36 4 observ.	51.750	16 20 30.91	-1.593	5 25 27 1.32229	9 9 30 0.43044	5 29 19 1.2779	2 26 34 0.98384
<i>d</i>	96 12 14.73 5 observ.	51.403	15 27 35.2B 5 observ.	-2.219	5 24 18 1.31863	9 9 23 0.50050	5 29 7 1.26839	2 25 3 0.98304
<i>f</i>	97 27 26.05 5 observ.	50.531	13 8 4.62 5 observ.	-2.600	5 23 9 1.31493	9 9 15 0.57056	5 28 56 1.25889	2 23 32 0.98225
<i>g</i>	98 50 14.14 5 observ.	50.496	13 4 44.74 5 observ.	-3.417	5 21 53 1.31714	9 10 44 0.57593	5 28 55 1.26091	2 22 7 0.98121
<i>h</i>	99 47 23.55	49.972	1 observ.					
<i>i</i>	100 32 11.72 2 observ.	49.718	11 0 00.30 2 observ.	-3.664	5 20 35 1.31380	9 8 54 0.64461	5 28 55 1.25714	2 22 7 0.98121
<i>n</i>	101 32 8.3 2 observ.	49.039	9 5 41.64 1 observ.	-4.001	id.	id.	id.	id.
<i>o</i>	101 38 33.05 4 observ.	48.993	8 57 57.32 4 observ.	-4.040	5 19 17 1.31046	9 7 5 0.71335	5 29 2 1.24949	2 21 7 0.98033
<i>p</i>	102 16 0.89 4 observ.	48.596	7 51 8.02 3 observ.	-4.257	5 18 41 1.3095	9 6 42 0.7354	5 29 2 1.24060	2 20 49 0.98000
<i>s</i>	103 12 40.49	1 obs.						
<i>w</i>	103 48 38.1 2 observ.	48.006	6 10 54.66 2 observ.	-4.779	5 17 34 1.30756	9 4 55 0.77947	5 29 10 1.23801	2 19 41 0.97898
<i>x</i>	104 7 42.13 2 observ.	47.990	6 9 6.03 2 observ.	-4.885	5 17 34 1.30756	9 4 55 0.77947	5 29 10 1.23801	2 19 41 0.9790
<i>y</i>	104 4 39.57	47.891	1 observ.					
β	104 47 56.5 2 observ.	47.479	2 observ.					
γ	104 49 3 75 5 observ.	47.405	4 27 0.21 6 observ.	-5.123	5 16 22 1.30556	9 3 29 0.82355	5 29 22 1.23761	2 18 57 0.97813
ϵ	110 5 23.82 7 observ.	45.392	1 32 45.01A 8 observ.	+6.871	5 11 27 1.30258	2 13 45 0.93112	5 29 42 1.22713	2 14 50 0.97282
ζ	111 46 29.64 2 observ.	44.932						
η	111 47 59.27 3 observ.	44.998	2 46 15.42 3 observ.	+7.431	5 10 44 1.30295	2 14 55 0.94733	5 29 25 1.22186	2 14 16 0.97191
θ	112 31 11.56 1 observ.							

Name of Star.	AR. media pro initio 1823	Præc. in AR.	Dec. med. pro initio 1823	Præc. in Dec.	Aberratio		Nutation	
					in AR. S. G. M.	in Dec. S. G. M.	in AR. S. G. M.	in Dec. S. G. M.
α Enckii Cometæ	115 25 0.87 7 observ.	43.119	8 44 30.29 7 observ.	+8.593	5 6 26 1.30517	2 21 50 1.04458	5 27 41 1.19023	2 10 51 0.96644
λ	115 42 39.21 3 observ.	43.055	8 57 29.4 2 observ.	+8.683		Stargard, New South Wales, 12th Oct. 1823.		
μ	115 52 19.28 2 observ.	42.979	9 12 25.94 1 observ.	+8.735				

v. *Remarks on HONES. Addressed to the Editor by a Correspondent.*

SIR,

An error in a *Table of Logarithms* may, indeed, chance to cause some trouble to a nautical and astronomical computer, once or twice in the course of twenty years; but a *bad razor* is a daily annoyance to seamen, as well as landsmen, to astronomers as well as gastronomers; and I therefore think it right to communicate to you some observations which have saved me a minute or two on an average every day of my life, for the last two or three years, and have spared me, perhaps, 100 out of 500 alternate motions of the hand and arm.

I have long been in the habit of using soap and water with a hone, in preference to the oil which is commonly employed; though I still doubt whether a constant immersion in oil might not, after all, keep the hone in the best possible order. But in any way that the hone is kept dry, its surface seems, in the course of a few months' use, to become hard or clogged, so that it will cut but very slowly, notwithstanding it may, at first, have possessed the requisite perfection of being soft enough to be scratched by a pin.

The remedy is to give it, every two or three months, a *new surface*, by a piece of common coarse *whetstone*, such as is used for scythes; and immediately after this *renovation*, a razor may be set in one minute as effectually as in ten by the same hone before the operation.

I am, Sir,

Your very obedient servant,

London, Jan. 1, 1825.

MISOPOGON.

vi. *Further Remarks on LUNAR TRANSITS, in illustration of MR. HENDERSON'S Paper.*

1. It must first be observed, that when a difference of longitude is expressed in time, the time intended is *sidereal*, and not solar.

2. Thus the difference of longitude is equivalent to the difference of *absolute* sidereal time at which the same star comes to the meridian of two places.

3. But the *local* sidereal time of the transit of any star is the same at all places; while the difference of the local sidereal time of the transits of two stars, whether at the same place or at different places, gives the difference of their right ascensions; and the same is true of the moon and a star, or of the moon at two places.

4. Let this difference of the moon's transit, in sidereal hours, be called a ; it will be expressed in degrees of space by $15^{\circ}a$, which will be the difference of the moon's right ascensions; and if the change of the moon's right ascension in an hour of solar time be h , and in an hour of sidereal time ϵh , the sidereal time required for this change will be $\frac{15^{\circ}a}{\epsilon h}$, ϵ being $= \frac{365.24}{366.24}$.

5. In this difference of absolute time will be comprehended the difference of longitude d , and the additional difference of local time a , so that $\frac{15^{\circ}a}{\epsilon h}$ will be equal to $d + a$, and $d = \frac{15^{\circ}a}{\epsilon h} - a$;

which agrees with Mr. Henderson's equation $d = aR \frac{15^{\circ}}{h} - a$.

If we employ the Nautical Almanac, where the moon's place is computed for apparent time, Mr. Henderson's Table may often be of use in expediting the calculation. Mr. Bouvard seems to have expressed the difference of longitude in solar time, instead of sidereal, since $9^m 20^s$ of solar time are equal to $9^m 21.5^s$ of sidereal, answering to $2^{\circ} 20' 22''$ of space.

6. With respect to the diversity noticed by Mr. Henderson, between the right ascensions deduced from the observations at Greenwich, made with the transit instrument and the mural circle, it must be remarked, that the Astronomer Royal does not attach the slightest

weight to the latter as accurate records of the transits of the celestial bodies; and that they are noted for some subordinate purposes only; so that the transit instrument is the only authority that bears on this question.

- vii. *Second Memoir on the Theory of Magnetism.* By MR. POISSON. Read to the Royal Academy of Sciences, 27th Dec. 1824. From the *Annales de Chimie*, for January, 1825.

The former memoir on this subject, which I read to the Academy about a year ago, contains a detailed explanation of the principles which form the basis of the application of mathematical analysis to this important part of natural philosophy. These principles are hypotheses, to which we are led by the consideration of the most general facts in magnetism, and which are afterwards confirmed by the comparison of the results of calculation with those of experiments. The analogy which is observed between magnetic attraction and repulsion, and the mutual actions of electrified bodies, inclines us at first to attribute these phenomena, in the case of magnetism as well as in that of electricity, to two fluids, each of which attracts the particles of the other, and repels with the same force its own particles. But we soon discover that these imponderable fluids cannot be disposed, in bodies capable of being affected by magnetism, as they are in bodies which conduct electricity. In the latter, the two electrical fluids, as soon as they are separated from each other, tend to escape at the surface, and may pass, in any given quantity, from one body into another. But the case is different with respect to the *boreal* and *austral* fluids; for these fluids never quit even the smallest bodies to which they belong, however powerful the magnetizing forces may be: whence it has been inferred, that within the substance of bodies that are magnetized, the two magnetic fluids undergo displacements to an insensible distance only, which are nevertheless sufficient to render their action sensible without the body, being repulsive for the one, and attractive for the other.

I have given, in my former memoir, the name of *magnetical ele-*

ments of a body to the extremely small spaces, in comparison with its whole volume, through which the boreal and austral fluids can be separately moved; I have made no particular supposition with regard to their form, nor to their respective disposition; and I have considered them as insulated from each other by means of intervals impermeable to magnetism. According to this mode of considering the intimate nature of magnets, the sum of the magnetic elements which they contain, is a fraction of their volume, that may vary in different substances susceptible of being magnetic, and may also depend on their temperature; and in this manner we may explain how two bodies of the same form, but of different substances, or at different degrees of temperature, may exhibit, under the influence of the same forces, magnetical actions of very different intensities.

Beginning with this theory, which is here but briefly recapitulated, I have deduced from it, in the first memoir, the equations which express, for all possible cases, the laws of the distribution of magnetism within bodies that are rendered magnetical by induction, and those of the attractions or repulsions which they exert on points given in position. It is now only an analytical problem to resolve these equations, in order to deduce from them results which may be compared with experiment; but such a resolution is only attainable in a very limited number of cases, paying regard to the different forms of magnets. That which I have taken for an example in the first memoir, and which admits a complete resolution, is the case of a solid or a hollow sphere, magnetized by forces of which the centres of action are distributed in any given manner, either without or within it. If we reduce these forces to a single one, that is, to the magnetic action of the earth, the formulæ which contain the solution become very simple. We deduce from it without difficulty the direction of the needle of a compass, produced by the neighbourhood of a sphere, magnetized thus by the influence of the earth. This deviation varies with the distance of the middle of the needle from the centre of the sphere, from the plane of the magnetic meridian passing through that centre, and from the plane passing through the same point, perpendicularly to the direction

of terrestrial magnetism. The laws of these different variations, obtained by calculation, agree with those which Mr. Barlow, Professor at Woolwich, has deduced from a numerous series of experiments which he has made on the subject. The theory explains also a very remarkable fact observed by Mr. Barlow, relating to the magnetic action of a hollow sphere; he found that the intensity of this action does not sensibly vary with the thickness of the metal, at least when the thickness is not very inconsiderable, and is not less than about one thirtieth of an inch, for a sphere of ten English inches in diameter; whence he has inferred that the magnetism is confined to the surface of the magnetized bodies, or that it does not penetrate them beyond a very small depth. It appears, however, from the calculation, as founded on the distribution of two fluids throughout the mass of the magnets, that the action of a hollow sphere is very nearly independent of its thickness, as long as the proportion of this thickness to the radius is not expressed by a very small fraction, the value of which is different for different substances; a result which agrees perfectly with the experiment.

This remarkable agreement affords a very important confirmation of the accuracy of the analysis, and of the theory of magnetism on which it is founded. We might, however, be desirous of a still more diversified comparison of the theory with the phenomena; and for this purpose I have endeavoured to resolve the general equations of the first memoir, as applied to bodies not having a form so simple as that of the sphere. I have found, for example, that these equations may be resolved in a very simple manner for any elliptic spheroid, provided that the force which produces its magnetism be constant in magnitude and in direction throughout its extent; which happens with respect to the terrestrial magnetism. This solution is the object of the first paragraph of the memoir which I now offer to the Academy.

After having given the formulæ relating to a spheroid of which the axes have any imaginable relations to each other, I have particularly considered the two opposite cases of spheroids extremely flattened and extremely elongated. A spheroid greatly flattened

may represent a plate of which the thickness varies very slowly near the centre, and decreases from that point to the circumference. Its action on points near its centre must be sensibly the same as that of any other plate of a constant thickness, and of a very great extent. With regard to such a plate, I must here remark, that in the extract of my first memoir, the action of a plate of great extent has been erroneously compared with that of a hollow sphere, of which the ray is supposed to increase without limit; for the portion of the spherical surface most remote from the attracted point is as much greater in extent as the actions of all its points are weaker, so that its total action is a finite quantity, which is not to be neglected, as I had supposed. [Whether or no this correction was suggested by the doubt expressed in the translation inserted in these collections, the ingenious author has not thought it necessary to inform us; but the having been able to afford a useful hint, to a mathematician like Mr. Poisson, is an occurrence too flattering to the translator's vanity to allow him to pass it by in silence.]

In a similar manner we may assume that a spheroid greatly elongated approaches very near to the form of a needle or bar, of which the diameter decreases from the middle to the extremities, varying at first very slowly; and its action on points near its middle can differ but little from that of a bar, of which the diameter is constant, and very small in proportion to its length. When, therefore, we have experimentally observed the actions of a bar, or a plate, magnetized by the influence of the earth, on points very near the middle points of these bodies, we may compare our theory with observation in this new point of view. In order to facilitate this comparison, I have taken care to enunciate distinctly, in my memoir, the principal consequences of the calculation, which appear to be most deserving of experimental confirmation.

The second paragraph of the memoir relates to a question which is curious with regard to the theory, but still more important in a practical point of view, and which has lately excited much attention in England. I allude to the means of destroying the deviations to which the compass is subject on board of a ship, and which are occasioned by the magnetic action of the guns, the anchors, and

the other masses of iron by which it is surrounded. All these bodies are magnetized by the influence of the earth; in this state they act on the compass, and cause it to deviate sometimes very considerably, the deviations derived from this cause having amounted to 20° on either side of the natural direction of the needle, and even to near 40° in a voyage to high northern latitudes. They vary in the same place according to the situation of the vessel with regard to that direction; they vary also when the direction of terrestrial magnetism varies, with the latitude; so that it was desirable to find a method of destroying them applicable to all possible directions of the action of the earth, with regard to lines in certain fixed positions within the vessel. The method proposed by Mr. Barlow has been adopted in several voyages of considerable length; and if it has not entirely annihilated the deviations of the needle, it has at least confined them within narrow limits; and it has hitherto been judged sufficient for the occasions of navigation. It consists in placing near the compass a plate of iron, which is also magnetized by the action of the earth. It is placed in such a manner that the needle shall assume and preserve, in all the directions of the ship, a direction parallel to that of another horizontal compass, placed on shore, at a sufficient distance from the vessel to be out of the reach of its magnetic influence. Mr. Barlow supposes that, by trials, it will always be possible to find a position for the plate which will give it this property; and when it has been determined, he fixes the plate, and keeps it always in the same situation. If the other masses of iron contained in the vessel do not undergo considerable displacements, and if in reality the deviations have been annihilated in all directions at the point of departure, it is evident that they will continue to be neutralised throughout the voyage, notwithstanding the changes of the intensity and the direction of the terrestrial magnetism: and this is easily seen when we consider, on the one hand, that all the bodies, susceptible of being rendered magnetic, which form a part of the vessel, including Mr. Barlow's iron plate, being magnetized by the action of the earth, the intensities of their magnetic actions will increase or decrease in the same proportion as this force: and on the

other hand, that this force, remaining parallel to itself in the whole extent of the vessel, the change of its absolute direction can produce no other effect than the change of the situation of the vessel, with regard to this same direction. It must, however, be remarked, that we here suppose that the ship must be imagined to have been submitted, at the time that the adjustment is made, to a rotatory motion, not only in a horizontal plane round a vertical axis, but also in a vertical plane round a horizontal axis ; not that the compass can ever be afterwards employed in such a situation, but since the inclination of the magnetic force may change without limit during the voyage, so that the relative change of direction is the same. [And it is unnecessary to remark, that such a previous experiment as this was never contemplated by the inventor of the apparatus.]

According to these considerations, the question, which is the subject of the last paragraph of the memoir, is reduced to the inquiry, whether it is possible to destroy identically, that is to say, for all possible directions of terrestrial magnetism, the deviations of a horizontal needle, derived from bodies magnetized by the influence of the earth, by adding to them a piece of iron, which is to be magnetized by the same cause. This requires, first, that the magnetism should have the same degree of mobility in the piece of iron, and in each of the other bodies ; but we may admit that the coercive force may be very weak, so that the distribution of the magnetism may be always conformable to the actual direction of the action of the earth ; a supposition which seems to be but little removed from the truth, with regard to the magnetic substances which are found on board of ships. With respect to the form of the bodies acting on the compass, I have supposed, in order to arrive at a complete solution of the proposed question, that all these bodies are spheres, either solid or hollow, of any given diameters and thicknesses, sufficiently distant from each other to allow us to neglect their mutual actions, and disposed in any assignable manner around the magnetic needle. Of such a system of spherical bodies, magnetized by the influence of the earth, I have determined the action on a given point, in order to see if, by properly fixing the

magnitude and the situation of one of them, one might not destroy the deviatory action of the whole system, with respect to a horizontal compass placed at that point ; setting aside its reaction on the different bodies of the system.

The formulæ of my memoir show immediately that this action can never vanish for all the directions of the magnetizing force ; consequently the duration of the oscillations of the needle will always be affected, even when there is no alteration in its direction. In order that the horizontal compass may be free from deviation, it is sufficient that the horizontal results of the action of the earth, and of the action of the system of magnetized bodies, should coincide with each other for all directions of the terrestrial magnetism. Now we find that this coincidence is only possible when a certain quantity, depending on the magnitude of the given spheres, on their mutual differences, and on their distances from the compass, and from the horizontal plane which contains it, is either positive or evanescent, and when another quantity, depending on the same elements, vanishes ; and reciprocally, when these two conditions are fulfilled, we may produce the required coincidence, by adding a single sphere to the given system. Its magnitude, and the place of its centre, may still remain undetermined ; but the direction of one or more right lines drawn through the centre of the compass, on which it must be placed, will be given ; and its distance from the needle will depend on the diameter assigned to the sphere, and will be proportional to it. This latitude of the solution of the problem depends on the identity of the action of a sphere on a given point, while its centre moves on a line passing through the point, and at the same time its diameter increases or diminishes in the same proportion as the distance from the point. If the given system of spheres consists of a single one only, its centre, and that of the added sphere, must be situated in the horizontal plane which contains the needle, and the right lines drawn from the two centres, to its middle point, must be at right angles to each other ; and it will be necessary that the lengths of the lines should be proportional to the diameters of the spheres ; in this case the compass will constantly retain its natural direction, when the system of the two

spheres is made to turn round its point of suspension; which it would be easy to verify by experiment.

The example here considered, shows that *it is not always possible* to destroy in every direction the deviations of a magnetic needle, by *adding a new body* to the whole of those which have occasioned that deviation. Although we cannot assign, for bodies of all possible forms, such conditions as have been laid down for spherical bodies, we may at least determine, for all cases, the number of these conditions. When a system of bodies, magnetized by the action of the earth, and mutually influencing each other, affects the needle of a compass, it is necessary, in order that the horizontal direction of the needle may be constantly annihilated, that the system should correspond with equations belonging to the form and disposition of these bodies, which, in the most general extent of the problem, will be five in number. If among these bodies there is a sphere of given diameter, but indeterminate in its position, we may dispose of the three co-ordinates of its centre in such a manner, as to satisfy these five equations, and thus reduce them to two equations of condition; and, besides, there will be other conditions which must be fulfilled, in order that the values of the unknown quantities may be real. If the moveable body, instead of being a sphere, is, for example, a circular plate, of a given diameter and thickness, we may dispose of the three co-ordinates of its centre, and of the two angles which serve to determine its direction; we shall thus have as many indeterminate quantities as there are equations to be satisfied, and there will only remain the conditions necessary for the reality of the values of these five unknown quantities. The use which has been made of this method, on board of ships, having greatly diminished the deviations of the compass, it must be inferred that, in the ordinary disposition of the masses of iron which are present, the conditions relative to this system of bodies are nearly fulfilled; but *we can never be certain that other cases will not arise*, in which the addition of a single body, of determinate form and dimensions, *would not* be sufficient to destroy the deviations of the needle, or even to *reduce them to moderate limits*, especially if the dip should change very considerably in the course of the voyage.

Mr. Barlow has also proposed to employ his instrument in a manner which may be called the reverse of that which has been here described. Before the ship sails, he finds, by trial, a position of the plate fixed near the compass, such that, in all directions of the ship, it produces the same deviation with that of the ship itself, and in the same direction. Hence he infers, that if the deviations are not very great, the united actions of the iron and of the plate will occasion deviations about twice as great as those which either cause would produce alone. So that when the true variation is required in the course of the voyage, the observation being made twice in succession, once with and once without the plate, the difference of the two directions gives a measure of the effect; and we obtain the true natural variation, by adding this difference to the variation first observed, when the employment of the plate has diminished it, and by subtracting the difference, when the plate has increased the variation.

In order to form an estimate of the degree of generality of this mode of correction, I have inquired whether we could produce with a single sphere, for every direction of the earth's magnetism, the same deviations in a horizontal needle as are due to a system of spheres given in magnitude and in position, and magnetized, as well as the required sphere, by the influence of the earth. The investigation shows that this is only possible when the bodies fulfil, as in the former case of the destruction of their effect, two separate conditions; a certain quantity depending on the magnitudes and the dispositions of the given spheres must become evanescent, and another quantity which, in the former case, was to be positive or evanescent, must now be evanescent or negative. This method, therefore, is not more extensively applicable than the former; its application is less simple; and it becomes impracticable when the deviations are extremely great, though this is precisely the case in which the remedy is most wanted.

But when all the given spheres have their centres in the same horizontal plane which contains the compass, it is always possible to destroy their effect, or to imitate it by means of a single sphere placed in a proper situation. Its centre must be in the same plane with the rest, and we may find the directions of two right lines

meeting at right angles in the middle of the needle, on one of which the sphere must be placed, in order to destroy the effect of the other, and on the other, in order to imitate it; its distance from the compass depending, in both cases, on the magnitude which we may choose to assign to it.

I shall conclude this extract with the formulas relating to the action of a sphere magnetized by the influence of the earth, from which we may deduce, by very simple calculations, the results contained in the second paragraph of my memoir, which have already been explained.

Let a be the radius of the sphere; r the distance of its centre from the given point on which it acts; x, y , and z , the three co-ordinates of that centre, referred to three orthogonal axes, passing through the given point; α, β , and γ , the results of the action of the sphere reduced to those axes; and X, Y , and Z , those of the action of the earth expressed in the same manner; we shall then have:

$$X = \frac{-ka^3}{r^3} \left(\alpha - \frac{3\alpha x^2}{r^2} - \frac{3\beta yx}{r^2} - \frac{3\gamma zx}{r^2} \right)$$

$$Y = \frac{-ka^3}{r^3} \left(\beta - \frac{3\beta y^2}{r^2} - \frac{3\alpha xy}{r^2} - \frac{3\gamma zy}{r^2} \right)$$

$$Z = \frac{-ka^3}{r^3} \left(\gamma - \frac{3\gamma z^2}{r^2} - \frac{3\alpha xz}{r^2} - \frac{2\beta yz}{r^2} \right).$$

The quantity represented by k being a fraction depending on the nature of the sphere, and apparently differing little in general from unity. When the sphere is hollow, we must substitute for this quantity, $\frac{(a^3 - b^3)(1 + k)k}{(1 + k)a^3 - 2k^2b^3}$, in which b is the interior semidiameter of the shell; this factor also differing very little from unity, except when the thickness, $a - b$, is extremely small in comparison with a .

In order to compute the deviation of a needle by these formulæ, we must consider its middle point as the beginning of the co-ordinates, and neglecting its length in comparison with r , the values of the forces X, Y , and Z , with regard to the two poles, may be considered as equal, and having contrary signs.

ART. XIV.—MISCELLANEOUS INTELLIGENCE.

I. MECHANICAL SCIENCE.

1. *Improvements in Microscopes.*—Dr. Goring has caused Mr. W. Tulley, of Islington, to execute a triple acromatic lens of .333 inches sidereal focus, and .2 inches aperture, and another of only .2 inches focus, and .11 inches aperture. Used as single lenses, these constitute the utmost perfection to which magnifying glasses can be brought by artificial combination, but their power is too low to be extensively efficient. Applied, however, as object glasses, to a compound microscope with various apertures, and eye glasses, according to the nature of the body under examination, they render the instrument *equal to single lenses of the same amplifying power* in its capacity of shewing the most difficult test objects,—a degree of superiority which no compound instruments in which a magnified image of an object is viewed, instead of the object itself, have ever yet attained * ; and in consequence of which the luxurious accommodation of their large field of view, and the facilities they afford for illuminating opaque objects, have hitherto been justly rejected by the most distinguished naturalists in favour of the simple single lens. In justice to the celebrated Mr. Troughton, to whom science is so profoundly indebted on so many accounts, it is proper to observe, that he was the person at whose suggestion achromatic lenses were first made by Mr. Tulley, and which were intended to be applied as the object glasses of the microscopes of the Greenwich circle. The lenses in question were a little more than one inch focus, and a quarter of an inch aperture, but were rejected by Mr. Troughton, as no better than common ones, at least for the purpose they were intended for, and with the greatest

* Mr. Herschel, in a most original and masterly paper in the *Transactions of the Royal Society* for 1821, part 2, has ascertained the true theoretical curves for giving the smallest quantity of central spherical aberration in a magnifying glass composed of two lenses. Dr. Goring instigated Mr. Cornelius Varley to make that combination represented at fig. 5, in the plate attached to the paper alluded to, on such a scale as to give a focus of only 1-6th of an inch, with 1-15th inch of aperture. This forms the best object-glass for a compound microscope which can be made, (excepting the achromatus,) to which it approximates very much in point of distinctness; it however confuses the outline of delicate and minute transparent objects with a fringe of colour which is very prejudicial to vision. As a single microscope it performs admirably, and may probably be executed on a much smaller scale than 1-6th inch focus, so as to give a very useful power; as it is, it shews the texture of mother-of-pearl, and the fine lines or flutings on the feathers of moths and butterflies, and other test objects which require a power three or four times higher to be rendered visible in the *common* compound microscopes of commerce.

propriety; for though the chromatic aberration in them was in a great measure subdued, the spherical aberration was unaltered; their distinctness, therefore, was no greater than that of a common lens of the same angle of aperture. An immense difficulty remained to be overcome, *viz.*, the distinction of the aberration of figure, together with that arising from dispersion, which has now been effected. It is but a small point gained to render these lenses free from colour, for they may, notwithstanding, be no better, or even a great deal worse, with regard to distinctness, than common ones, as is the case also with the chromatic object glasses of telescopes.

Mr. J. Cuthbert has also (under the direction of Dr. Goring) constructed a reflecting microscope, on the principle of that invented by professor Amici, of Modena, which, *in its original condition and dimensions, viz., with an objective metal of $2\frac{1}{2}$ inches sidereal focus*, is good for nothing, notwithstanding the pompous eulogiums which have been bestowed on it; being unable to shew any but the most common and easy objects, as can be demonstrated by a very excellent one of the kind previously made by Mr. Cuthbert, in which the objective metal has a truly elliptical figure, &c.* Dr. Goring was of opinion that the principle of the instrument was good, but that the failure in the performance arose from the object metal of $2\frac{1}{2}$ inches focus, with a tube 12 inches long, forming an image which was only about three times larger than the object, so that all the rest of the requisite power had to be obtained by very deep eye-glasses: he accordingly planned both the optical and mechanical arrangements for another instrument, which has an objective metal of only .6 inches sidereal focus, and .3 aperture, with a tube 5 inches long. This being executed by Mr. Cuthbert, was found to perform extremely well, and to exhibit any objects which could be seen with the single microscope, to which it seemed equal, power for power.

Dr. Goring has also caused a *diamond lens* of $\frac{1}{20}$ th inch focus, to be executed by Mr. Andrew Pritchard, of 51, Upper Thornhaugh Street, (assistant to Mr. Cornelius Varley, under whose auspices it was worked.) A diamond is well known to be the most refractory body in nature, at least it is the most difficult which could be selected to receive a spherical figure; yet it is possible to form a lens of it, as the event has shewn, and (if we neglect the obstacles which present themselves in working it) seems precisely the substance which is most adapted to form a small microscopic magnifier, for its refractive power is nearly double that of glass, while its dispersive power is no greater than that of water; its extreme hardness also ultimately causes it to receive the most exquisite figure and po-

* Mr. Dolland, it is said, has also executed one with the same results.

lish. Thus a diamond lens will always magnify very nearly twice as much as a glass one ground in the same tool, while its spherical and chromatic aberrations are no greater with a given aperture. The lens in question is plano-convex, and was ground in a tool which would have made a glass one of $\frac{1}{10}$ th inch focus, to which it is precisely similar in size and outward figure: it carries the same aperture also equally well, (only with the peculiarity of magnifying twice as much, being very nearly $\frac{1}{20}$ th inch focus.) Most unfortunately, several flaws have appeared in the stone, which is, moreover, at present imperfectly polished; it is nevertheless capable of acting very well, and shews the most difficult objects both as a single magnifier, and as the objective glass of a compound instrument; it has been used with as much as $\frac{1}{20}$ th inch of aperture, and exhibited to many individuals who are perfect judges of these things. Mr. Cornelius Varley (who is exceeded by no man in his skill in working small lenses) proposes to make a lens of diamond in the same tool which would form a glass one of $\frac{1}{60}$ th inch focus, (being the smallest lens which can well be made,) which will, of course, have a focus of about $\frac{1}{100}$ th of an inch. Farther particulars concerning the microscopes mentioned in this notice, may be had by application to the artists who have been here designated, and a full and particular account of each will be given in a work on the microscope, which is preparing for the press.

2. *Capillary Attraction.*—M. Gilleron says, “If a capillary tube be introduced into mercury, the metal will remain in the tube below the exterior surface. If then the tube be carefully raised, without taking it out of the mercury, the surface of the mercury in the tube may be raised to the level of that without. Operating very carefully, it may even be raised still higher; its surface will then become concave, the nature of the curve apparently approaching that of the catenarian curve, which I believe also to be that of liquids which in capillary tubes are raised above the level of the external surface. If then the tube be depressed a little, the convex surface may be again given to the mercury in the tube, without its level being depressed below that of the external portion. It appears to me, therefore, that the surface of liquids in capillary tubes is an accessory circumstance, and has no direct influence on the elevation or depression of the liquid.”—*Bib. Univ.* xxvii. 209.

3. *Embossing on Wood.*—A new and ingenious method of embossing on wood has been invented by Mr. J. Straker. It may be used either by itself, or in aid of carving, and depends on the fact that if a depression be made by a blunt instrument on the surface of wood, such depressed part will again rise to its original level by subsequent immersion in water.

The wood to be ornamented, having first been worked to its pro-

posed shape, is in a state to receive the drawing of the pattern ; this being put in, a blunt steel tool, or burnisher, or die, is to be applied successively to all those parts of the pattern intended to be in relief, and at the same time is to be driven very cautiously, without breaking the grain of the wood, till the depth of the depression is equal to the subsequent prominence of the figures. The ground is then to be reduced by planing or filing to the level of the depressed part ; after which the piece of wood being placed in water either hot or cold, the parts previously depressed will rise to their former height, and will thus form an embossed pattern, which may be finished by the usual operation of carving.—*Trans. Soc. Arts.* xlii. 52.

4. *Drawing of Iron Wire facilitated.*—A manufacturer of iron and steel wire observed that the wire which had been pickled (a process requisite in the course of the drawing) in an acid liquor, the temperature of which had been raised by the immersion of some hot ingots of brass, passed through the holes in the drawing plates with remarkable facility, in consequence of the precipitation of a portion of the copper in the liquor upon its surface ; it required to be annealed much less frequently than before, the copper apparently preventing the action of the plate so as to gall or fret the wire. In consequence of this fact, the same person has constantly availed himself since of the use of a weak solution of copper in iron and steel wire-drawing. The thin coat of copper is entirely removed in the last annealing process.—*Tech. Rep.* vii. 161.

5. *Hawkins' mode of preparing Emery.*—Mr. Hawkins finding that the emery sold in the shops was totally inefficient for the purpose he had in view, namely, grinding two flat surfaces of hard cast steel accurately, thought of applying a process he had seen for washing over *diamond dust*, to emery ; and to be certain that his emery was of good quality, he purchased of an emery-maker a quantity of those small lumps, or grains, which had longest withstood the action of the cast-iron runners and bed, and thus ensured the hardness of the emery ; these pieces were reduced to powder in a cast-iron mortar, and then separated into different portions by sieves.

He then washed over the finest emery thus obtained, using oil instead of water, as in the usual process, the former holding it in suspension for a much longer time ; and in this way obtained a series of emery which had floated one, five, ten, fifteen, twenty, forty, and eighty minutes, amongst which he found every variety necessary for his purpose ; and keeping them in boxes, which were numbered according to the minutes they had floated, he could at any time prepare more of any one kind. In this way Mr. Hawkins readily attained his object, and ultimately by selecting those grains of emery which resisted longest the action of the pestle and

mortar, he obtained some so hard, as to be capable of cutting a ruby, when employed instead of diamond-dust.

Mr. Gill by grinding Greek emery-stone between two flat and hard steel surfaces, and washing off the lighter portions in oil, found that those which subsided in half a minute, when examined by a microscope, had entirely resisted the friction, and were perfectly crystallized sapphires.—*Tech. Rep.* vii 45.

6. *Power of Building Materials to resist Frost.*—In the xvii. volume of this Journal, p. 148, we gave an account of a process devised by M. Brard, for the estimation of the power possessed by building materials, of resisting the disintegrating action of frost and weather. He imitated this action by the spontaneous crystallization of a solution of Glauber's salt, the crystals producing the same effect upon the particles of the stones submitted to trial as the formation of ice would have done by the exposure of the same stone to cold after being moistened. The importance of M. Brard's process, and its great utility to architects, has been proved in France by MM. Vicat, Billoudel, Conrad, and de Thury, who in consequence consider the means as known, by which the power of any building-stone or material to resist the disintegrating action of frost or weather, may be ascertained in a few days.

In addition to the directions formerly given, the following have been added in the instructions drawn up by the French philosophers. The stone having been selected, the cubes cut, and the solution of sulphate of soda (saturated at common temperature) made, the solution is to be boiled, and whilst boiling freely, the specimens are to be introduced. The stones are to be boiled for half an hour and not longer: M. Vicat having shewn that afterwards the effect surpasses that of frost. The specimens are then to be withdrawn one after the other, and suspended by threads, so that they do not touch each other, or any thing but the thread; a vessel containing some of the clean solution in which they have been boiled is to be placed beneath each, after which the vessel and its accompanying specimen are not to be separated.

If the weather be not too moist or too cold, the specimens will be found covered in 24 hours with small white saline needles. They are then to be plunged, each into the particular portion of solution beneath it, when the needles fall off, and are again to be suspended in the air. A repetition of this process is to take place each time the needles are well formed. If the stone under trial is capable of resisting the action of frost, the salt will remove nothing from it, and neither grains, nor scales, nor fragments, will be found at the bottom of the solution beneath. On the contrary, with a stone which gives way to the weather, it will be seen that even on the first day the salt will remove particles from it,

the cube will lose its angles and edges, and ultimately there will be found at the bottom of the vessel all that has been detached during the trial. The trial should be concluded at the end of the fifth day after the salt has first appeared in crystals. The formation of crystals may be facilitated by moistening the stone as soon as they have appeared on any one point; this may be repeated five or six times a-day.

Great care should be taken that the saturation of the water by the salt be effected at common temperatures. The experiments of M. Vicat and others have demonstrated, that stone, which resists perfectly the action of frost or of cold solution of sulphate of soda, gives way entirely when exposed to the action of a saturated hot solution: the same is the case frequently also, if the trial be continued beyond the fifth day. Mortars and bricks which had withstood ten winters gave way to saturated hot solutions; and M. de Thury found that lias and other stones which had resisted the weather for ages, were disintegrated by the same excessive kind of trial: from which it may be concluded, that stones which *can* resist these trials would scarcely undergo any change by exposure to weather for any length of time.

If it be required to estimate comparatively the power possessed by two or more kinds of stone to resist the action of frost; the portion of matter separated from them, and lying in the solution beneath, is to be collected, washed, dried, and weighed; and the weight indicates the proportion in which the samples tried would suffer by exposure to weather and frost.—*Ann. des Mines*, ix. 741.

7. *Economical Method of warming Manufactories, &c.*—Mr. Bewley, of Montraith, Ireland, has contrived a method of warming his cotton-mill by the waste heat of a lime-kiln, apparently in a very effectual manner. The kiln is closed in at the top by a cover of cast-iron, from the middle of which a cast-iron flue proceeds, carrying off the carbonic acid, smoke, &c. This is surrounded by a larger pipe intended for the conveyance of air, which commencing at a brick enclosure surrounding the top of the kiln, continues to the upper part of the building, and has openings from it into the rooms of the mill, for the passage outwards of the air that has been heated during its ascent through it. Altogether, therefore, it resembles in principle Perkins' stove. The particular advantages mentioned by Mr. Bewley are, that a very cheap kind of fuel may here be used, scarcely fitted indeed for any other purpose, namely, culm, cinders, &c., and that the lime produced will in almost all situations pay the expenses, and in many afford a profit. The heat also is a very steady one, and is continued during the night as well as the day, by which changes of temperature, sometimes injurious, are avoided. The work-

people also are more willing to come to work, having warm workshops to come to. The method has the merit of being a very safe one.

The cotton-mill warmed contains five rooms, the four upper of which are supplied with warm air by the means described. The rooms are fifty feet long by twenty broad: the general temperature is said to be about 80° Fah. The kiln is called a small one, being eleven feet deep, seven feet greatest diameter. It is fed with limestone and fuel by a door in its cover, and is drawn twice in 24 hours. If a very steady heat be required, it is recommended to be drawn three or four times in 24 hours.

Where lime may not be wanted, the burning of bricks or tiles, or even clay for manure, is suggested, as admitting of the same arrangement.—*Trans. Soc. Arts*, xlii. 134.

8. *Method of consuming the Smoke of Steam-boiler Furnaces, &c.*, by Mr. Chapman.—Mr. Chapman's process consists in the introduction of air into the furnace beyond the fire; but he uses peculiar means of heating this air before it is introduced, and this renders the combustion of the smoke more ready and perfect. He says, "To heat the air before its admission into the furnace. This I do by casting the grate-bars hollow from end to end, so that they form a series of parallel tubes, which open into two boxes, one placed in front, and the other behind the grate. In the front box, directly underneath the fire-door, I make a register to open and shut, to any extent, at pleasure. The other end I connect with the brick-work directly under the fire-bridge, which fire-bridge I make *double*, with a small interval between, say one inch; the interval to go across the furnace from side to side, and rather to incline forward, or toward the fire-door, so as to meet and reverberate the smoke on to the ignited fuel in the grate, which causes it to inflame and become a sheet of bright fire under the bottom of the boiler." Consequently when the register is open, air passes along the bars of the grate, becomes heated in its passage, and is then thrown on to the hot smoke, causing its inflammation and combustion.

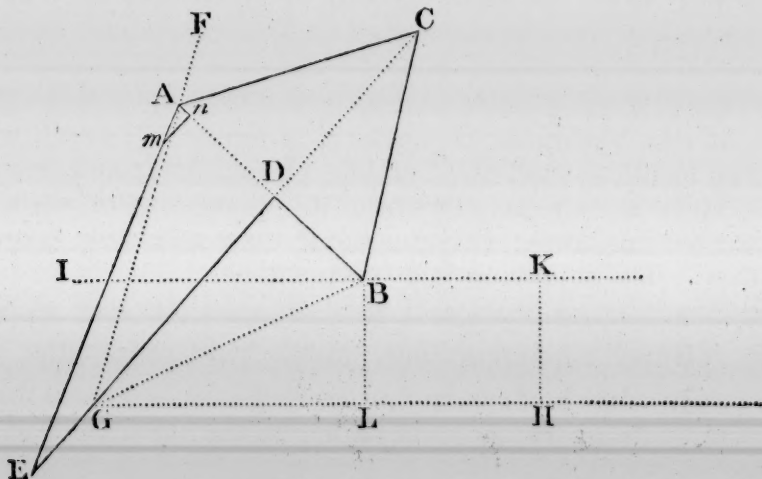
Mr. Chapman found that though his plan answered perfectly when the fire-door was closed, yet it was inefficient when the door was opened for the introduction of coals, so much cold air passing in as to prevent the due effect of the arrangement. To obviate this, he fed his fire with fuel in a different manner. "I adopted a cast-iron hopper above the fire-door, with a type at the bottom that has two pivots on one side, and opens at the other; one pivot goes through the end of the hopper, and has a counter-lever to keep the type shut, when a sufficient quantity of coal for a charge is on it. The top of the hopper is covered with a lid, which I shut down during the time of firing; then, by lifting the

lever which opens the type inside, the coals slide down on to the fore end of the grate bars, which is only the work of a moment. It is evident that no quantity of cold air can thus get into the furnace; in fact, it is not possible for any person that does not see the operation of firing, to know when fresh fuel is added by looking at the top of the chimney. The smoke that issues is never more than a light grey, just perceptible, but in a general way is not seen at all."

The coals last admitted are allowed to lie until partially coked, and just before a fresh supply is admitted, are pushed farther along the grate by a tool for the purpose, which remains constantly in the furnace: it is merely a plate of iron, about four inches broad, and as long as the grate is wide: it has a handle long enough to be used with both hands, which comes through a hole in the bottom of the door: when not in use it is drawn close up to the door. There is also another hole about one inch in diameter in the fire-door to look through; a circular piece of iron hangs before it by a loose rivet.

The certificates of persons who have witnessed the effect of this application speak to its entire success. Those who, previous to the erection of the engine, apprehended a nuisance, have been agreeably disappointed, smoke being seldom seen. On the application of fresh fuel, the smoke assumed the appearance of a light-grey vapour, which in a few seconds became almost invisible. On opening the fire-door in the usual way, dense black clouds of smoke rolled out of the chimney, but they ceased on closing the door again. An unlooked-for advantage also is, that the grate-bars appear to last longer when thus constructed.—*Trans. Soc. Arts*, xlii. 32.

9. *Graphical trisection of an Angle.*—The following instrument, for such it is, is supposed to be new, and may prove useful to artificers and others who have occasion to trisect any angle.



Having a plate of sheet metal, or other convenient material, let $A C B$ be an equilateral triangle; divide one of its sides, $A B$, into two equal parts, as at D , through which draw the line $C E$, until it meets, as at E , another line $A E$, which latter makes an angle of 70° with $A B$; consequently, the angle E will be 20° . The periphery of the rectilineal figure, $F A B C D$, will represent the instrument in question.

To illustrate its application, suppose $F G H$ the angle to be trisected; raise on one of its sides a perpendicular, as $K H$, equal to $A D$ or $B D$, and through the point K draw $I K$, parallel to $G H$; then place the instrument between this line $I K$, and the other leg $F G$ of the angle given, so that the line $A B$ touches these two lines at its extremities, whilst the line $D E$ passes by the apex G of the same angle: when in this position, the angle $A G D$ will be equal to one-third of the given angle $F G H$; for it is evident, that if from B the perpendicular $B L$ be drawn to the line $G H$, and that the points B and G be joined by the line $B G$, that the rectangular triangles, $L G B$, $D G B$, $A G D$, will be equal, the sides, $A D$, $D B$, and $B L$ being equal, and consequently, the opposite angles at G will also be equal.

The reason why the angle C is made equal to 60° , and the angle E to 20° , is as follows:—If the angle given was less than 60° , and consequently, its third smaller than the angle E , the perpendicular $D E$ would, when employed as above, be too short to extend to G , and the operation, in this case, would be a little more complicated: instead of searching for the third of the angle given, the operation is to be commenced by finding the third of its supplement, which, once obtained, need only be subtracted from an angle of 60° to give the third of the angle required; now by making one of the angles of the instrument itself 60° , this graphical subtraction is very much facilitated, and the operation rendered so simple as to require no application. The line $m n$ indicates a small triangle, $m A n$, which it is convenient to remove from the instrument, that the point A may have an acute angle instead of the obtuse angle $C A E$.—*Bib. Univ.* xxvii. 169.

10. *Secret Writing*.—Mr. Allsop describes, in a letter to the Editor of the Technical Repository, a mode of secret writing which he has long used for making private notes and memorandums. It is simply to substitute the preceding or succeeding letter for the one used; thus, for the letter a substitute b , for b , c , and so on. The following is a specimen:—

“ Sir, Among the numberless inventions adopted for Secret

“ Sir, Bnpoh uif ovncfsmtt jowfoujpot bepqufe gps Tfdsfu

Always addressing and concluding the letter in the usual manner, to prevent a discovery, or giving a key to the cipher.—*Tech. Rep.* vii. 174.

11. *Ink similar to China Ink.*—M. Fontenelle says, that an ink, equal in colour and goodness to China or Indian ink, may be made by dissolving six parts of isinglass in twelve of water, one part of Spanish liquorice in two of water, mixing the solutions whilst warm, and incorporating with them one part of the best ivory-black, using a spatula, and adding but small portions at once. When the mixture is complete, it is to be heated in a water bath, until so much water is evaporated as to leave a paste which may be moulded into any required form, and then the drying completed.

12. *English Opium.*—Messrs. Cowley and Stains still continue to grow poppies for opium*, and the following result will shew with what success this branch of agriculture is likely to be attended. In the year 1823, they collected as much as 196 lbs. of opium from poppies growing on twelve acres, one rod, and thirteen poles of land. Its character was such in the market, that it sold at two shillings per pound above the best foreign opium, and they believe that nothing but the carelessness of cultivators is likely to bring it into disrepute. One of the most positive directions given to those employed in collecting it, is to avoid the fall of petals, stamina, &c. into the receivers, and to take care if an implement falls to the ground, that it be properly cleaned from grit, &c., a small quantity of which would seriously injure the quality.

The expenses attendant upon the cultivation of the twelve acres, one rod, and thirteen poles of white poppies, and the extraction of the opium, seed, and extract, amounted to 274*l.* 1*s.* 9*d.*, of which above 103*l.* was paid to the labourers who collected the opium. The produce was as follows :—

	£.	s.	d.
Opium, 196 lbs., at 1 <i>l.</i> 10 <i>s.</i> 6 <i>d.</i> per lb.	298	18	0
Seed, 25 cwt. 1 qr. 22 lbs., at 12 <i>s.</i> per cwt.	15	5	3
Ditto unsold, about 5 cwt., worth	3	0	0
Extract, 381 lbs., at 1 <i>s.</i> 6 <i>d.</i>	28	11	6
Turnips, 10 acres, at 2 <i>l.</i> 10 <i>s.</i> per acre	25	0	0
	370	14	9

There is one remark respecting the soil brought into this kind of cultivation, so important, that we quote it at length. “A

* Quarterly Journal of Science, xv. 139.

porous subsoil appears to us as a circumstance of the first-rate importance, for where it consists of clay, our crops have invariably been inferior to those which have grown on such parts as were situated upon the sand, although assisted with more manure; and this too, when, owing to frost, no injury could be attributable to the treading of the sheep, when feeding off the turnips. So strong, indeed, is our conviction of the ill effects of an impervious subsoil, that we have no hesitation in saying, that however good the soil, or however dry it may appear, if it be situated immediately above clay, no profit can be extracted from it by the growth of poppies, so frequent will be the partial (or total) failure of the crop."—*Trans. Soc. Arts*, xlii. 9.

13. *Letter to the Editor of the Quarterly Journal of Science, Literature, and the Arts, concerning MR. JOB RIDER'S rotatory Steam-Engine.* By ANDREW URE, M.D., F.R.S., &c.

DEAR SIR,—In your 16th volume, there are two letters descriptive of the structure and performance of the above engine. The first letter is signed Job Rider, the second William Boyd. These letters were transmitted to me from Belfast, in order that I might send you an account of the engine, for insertion in your Journal. I accordingly forwarded the letters, after making two or three merely verbal corrections, which certainly did not, in the slightest degree, alter the sense. For the truth of this assertion, you yourself can vouch, in regard to Mr. Boyd's letter, since it was from his own holograph, that pages 269 and 270 of the above volume of the Journal were printed*.

It now appears that Mr. Boyd did not intend that his letter should be published; but as he gave me no hint whatever to this effect, I did not feel myself justified, as a mere organ of transmission, in withholding that letter which alone furnished an account of the performance of the engine. Towards the conclusion of this letter, there is an observation which has given offence to Messrs. Girdwood and Co., eminent engineers in this city. The engine which this company made on Mr. Rider's plan for the Highland Lad steam-packet, was some time ago taken out of the boat, (in which it gave no satisfaction,) and has been since mounted in their manufactory. Here I have seen it in action. It was working the powerful blast bellows of their foundry, turning a loam-mill, and impelling the turning lathes, as well as the boring-machinery of two extensive floors of their workshop. I have therefore little doubt, both from this evidence, and the well-established reputation of Messrs. Girdwood and Co., that the rotatory

* The types were set up from Mr. Boyd's original letter, on which no alteration had been made in the least affecting the sense.—ED.

engine owed its failure in the steam-boat, not to bad workmanship on the part of the Glasgow engineers.

I am, dear Sir,

Yours very truly,

ANDREW URE.

Glasgow, March 1st, 1825.

14. *On Paratonnerres, or Conductors of Lightning.*—A very interesting report on the subject of *Paratonnerres*, has been presented to the Royal Academy of Sciences by M. Gay-Lussac, in the name of a commission appointed specifically for the purpose, and an account of which has since been published in the *Annales de Chimie*, and more recently a translation has appeared in the *Annals of Philosophy*, for December, 1824. The paper is divided into two parts; one *theoretical*, and the other *practical*, and the information contained in it may be regarded as the most perfect we possess on the subject.

The theoretical part is introduced with some general observations on electric matter, and of conductors; that its velocity is at the rate of about 1950 feet per second; that it penetrates bodies, and traverses their substance, with unequal degrees of velocity; that the resistance of a conductor increases with its length, and may exceed that which would be offered by a worse but shorter conductor; and that conductors of small diameter conduct worse than those of larger. The electric matter also tends always to spread itself over conductors, and to assume a state of equilibrium in them, and becomes divided among them in proportion to their form, and principally to their extent of surface; and that hence a body that is charged with the fluid being in communication with the immense surface of the earth, will retain no sensible portion of it.

Gay-Lussac defines a paratonnerre to be a conductor which the electric matter prefers to the surrounding bodies, in order to reach the ground, and expand itself through it; and commonly consists of a bar of iron elevated on the buildings it is intended to protect, and descends without any *divisions* or *breaks* in its length, into *water* or *moist ground*. When a paratonnerre has any *breaks* in it, or is not in perfect communication with a moist soil, the lightning having struck it, flies from it to some neighbouring body, or divides itself between the two, in order to pass more rapidly into the earth.

The most advantageous form that can be given to the extremity of a paratonnerre is that of a very sharp cone, and *the higher it is elevated in the air*, other circumstances being equal, *the more its efficacy will be increased*, as is proved by the experiments of M.M. de Romas and Charles.

It has not been accurately ascertained how far the sphere of action of a paratonnerre extends, but in several instances, the more remote parts of large buildings on which they have been erected, have been struck by lightning at the distance of three or four times the length of the conductor from the rod. According, however, to the opinion of Charles, *a paratonnerre will effectually protect from lightning a circular space, whose radius is twice that of the height of the conductor.* By increasing, therefore, the altitude of a conductor, the space also which it will protect is augmented in proportion.

A current of electric matter, whether luminous or not, is always accompanied by heat, the intensity of which depends on the velocity of the current. This heat is sufficient to make a metallic wire red hot, or to fuse or disperse it, if sufficiently *thin*; and hence we may perceive the absurdity of some attempts which have been lately made, to protect ships, by *thin* slips of copper nailed to their masts. The heat of the electric fluid scarcely raises the temperature of a bar of metal, *on account of its large mass*; and no instance has yet occurred of an iron bar, of rather more than half an inch square, or of a cylinder of the same diameter, having been fused, or even heated red hot by lightning. A rod of this size would, therefore, be sufficient for a paratonnerre; but as its stem should rise from 15 to 30 feet *above* the building, it would not be of sufficient strength at the base to resist the action of the wind, unless it were made much thicker at that part. An iron bar, about three-quarters of an inch, is sufficient for the conductor of the paratonnerre.

According to Gay-Lussac, a paratonnerre consists of two parts, the *stem* which projects into the air above the roof, and the *conductor*, which descends from the foot of the stem to the ground. The stem he proposes to be a square bar of iron, tapering from its base to the summit, in form of a pyramid, and for a height of from 20 to 30 feet, which is the mean length of the stems placed on large buildings; the base should be about $2\frac{1}{2}$ inches square. Iron being very liable to rust by action of air and moisture, the point of the stem would soon become blunt; and therefore, to prevent it, a portion of the top, about 20 inches in length, should be composed of a conical stem of brass or copper, gilt at its extremity, or terminated by a small platina needle, two inches long. Instead of the platina needle, one of standard silver may be substituted, composed of nine parts of silver, and one of copper. The platina needle should be soldered with a silver solder to the copper stem; and to prevent its separating from it, which might sometimes happen, notwithstanding the solder, it should be secured by a small collar of copper. The copper stem is united to the iron one, by means of a gudgeon, which screws into each; the gudgeon is first fixed in the copper stem by two steady pins at right angles

to each other, and is then screwed into the iron stem, and secured there also by a steady pin.

The conductor should be about three-quarters of an inch square, and as before stated, should reach from the foot of the stem to the ground. It should be firmly united to the stem, by being tightly jammed between the two ears of a collar, by means of a bolt. The conductor should be supported parallel to the roof, at about six inches distance from it, by forked stanchions, and after turning over the cornice of the building, without touching it, should be brought down the wall, and to which it should be fastened by means of cramps. At the bottom of the wall, it is bent at right angles, and carried in that direction 12 or 15 feet, when it turns down into a well.

Since iron buried in the ground in immediate contact with moist earth soon becomes covered with rust, and is by degrees destroyed, the conductor should be placed in a trough filled with charcoal, in the following manner. Having made a trench in the earth, about two feet deep, a row of bricks is laid on their broad faces, and on them others on edge; a stratum of bakers' ashes (*braise de boulanger*) is then strewed over the bottom bricks, about two inches thick, on which the conductor is laid, and the trough then filled up with more ashes, and closed by a row of bricks laid along the top. Iron thus buried in charcoal, will undergo no change in 30 years. After leaving the trough, the conductor passes through the side of the well before alluded to, and descends into the water to the depth of at least two feet, below the lowest water line. The extremity of the conductor usually terminates in two or three branches, to give a readier passage to the lightning into the water. If there be no well at hand, a hole must be made in the ground, with a six-inch auger, to the depth of about 10 or 15 feet, and the conductor passed to the bottom of it, placing it carefully in the centre of the hole, which is then to be filled up with bakers' ashes, rammed down as hard as possible, all round the conductor. In a dry soil, or on a rock, the trench to receive the conductor should be at least twice as long as that for a common soil, and even longer, if thereby it be possible to reach moist ground. Should the situation not admit of the trench being much increased in length, others, in a transverse direction, should be made, in which small bars of iron, surrounded by ashes are placed, and connected with the conductor. In general, the trench should be made in the dampest, and consequently lowest spot near the building, and the water gutters made to discharge their waters over it, so as to keep it always moist. *Too great precautions cannot be taken to give the lightning a ready passage to the ground, for it is chiefly on this, that the efficacy of a paratonnerre depends.*

As iron bars are difficult to bend according to the projections of a building, it has been proposed to substitute *metallic ropes* in

their stead. Fifteen iron wires are twisted together, to form one strand, and four of these form a rope, about an inch in diameter. To prevent its rusting, each strand is well tarred separately, and after they are twisted together, the whole rope is tarred over again with great care. Copper or brass wire is, however, a better material for their construction than iron. If a building contain any large masses of metal, as sheets of copper or lead on the roof, metal pipes and gutters, iron braces, &c., they must all be connected with the paratonnerre, by iron bars of about half an inch square, or something less. Without this precaution, the lightning might strike from the conductor to the metal (especially if there should be any accidental break in the former), and occasion very serious injury to the building, and danger to its inhabitants.

Paratonnerres for Churches.

For a tower, the stem of the paratonnerre should rise from 15 to 24 feet, according to its area; the domes and steeples of churches, being usually much higher than the surrounding objects, do not require so high a conductor as buildings with extensive flat roofs. For the former, therefore, their stems, rising from three to six feet above the cross or weather-cock, will be sufficient, and being light they may easily be fixed to them without injuring their appearance, or interfering with the motion of the vane.

Paratonnerres for Powder-Magazines.

These require to be constructed with the greatest care. They should not be placed on the buildings, but on poles at from six to ten feet distance. The stems should be about seven feet long, and the poles of such a height, that the stem may rise from 15 to 20 feet above the top of the building. It is also advisable to have several paratonnerres round each magazine. If the magazine be in a tower, or other very lofty building, it may be sufficient to defend it by a double copper conductor, without any paratonnerre stem. As the influence of this conductor will not extend beyond the building, it cannot attract the lightning from a distance, and will yet protect the magazine, should it be struck.

Paratonnerres for Ships.

The stem of a paratonnerre for a ship, consists merely of a copper point, screwed on a round iron rod, entering the extremity of the top-gallant mast. An iron bar, connected with the foot of the round rod, descends down the pole, and is terminated by a crook or ring, to which the conductor of the paratonnerre is attached, which, in this case, is formed of a metallic rope, connected at its lower extremity with a bar or plate of metal, and which latter is connected to the copper sheathing on the bottom of the vessel.

Small vessels require only one paratonnerre; large ships should have one on the main-mast and another on the mizen-mast.

The late ingenious Mr. George Singer, in his excellent work on Electricity, proposed to have conductors *fixed to the surfaces of masts*, and the electric fluid conveyed by means of strips of metal over the deck and the sides of the vessel; but this arrangement on many accounts is highly objectionable, and the mode proposed by Gay-Lussac, or perhaps that commonly adopted in the British navy, of conveying the electric fluid from the mast head to the surface of the water, in a direct line, by means of a series of long copper links, is the best which has hitherto been devised.

It is allowed from experiment, that the stem of a paratonnerre effectually defends a circle of which it is the centre, and whose radius is twice its own height. According to this rule, a building 60 feet square, requires only a stem of 15 or 18 feet raised in the centre of the roof. A building of 120 feet by the same rule, would require a stem of 30 feet, and such are sometimes used; but it is better, instead of one stem of that length, to erect two of 15 or 18 feet, one placed at 30 feet from one end of the building, the other at the same distance from the other end, and consequently 60 feet from each other. The same rule should be followed for three or any greater number of paratonnerres. A plate is given in the *Annals of Philosophy* to illustrate this interesting subject more particularly.

15. *Influence of Copper, &c., on Magnetic Needles.*—Nov. 22, 1824. M. Arago communicated to the Academy of Sciences his experiments relative to the oscillations of a magnetic needle surrounded by different substances. He had ascertained that the copper rings with which dipping needles are generally surrounded exerted on the needles a very singular action, the effect of which was rapidly to diminish the amplitude of the oscillations without sensibly altering their duration. Thus when a horizontal needle suspended in a ring of wood by a thread without tension, was moved 45° from its natural position, and left to itself, it made 145 oscillations before the amplitude was reduced to 10° . In a ring of copper, the amplitude diminished so rapidly that the same needle, removed 45° from its natural position, only oscillated 33 times before the arc was reduced to 10° . In another ring of copper, of less weight, the number of oscillations between the arcs of 45° and 10° were 66. The time of the oscillations appeared to be the same in all the rings.

In the ring of wood	145 oscillations from 45° to 10° .
————— copper	33 45° „ 10° .
In a lighter copper ring	66 45° „ 10° .

16. *Intensity of Electro-dynamic Force.*—M. Bequerel has ascertained, by experiments, that the intensity of the electro-dynamic

force in a metallic wire joining the two poles of a voltaic pile, is constant for all points of the wire.

II. CHEMICAL SCIENCE.

1. *Variation of Boiling Points—Increased Production of Vapour.*—It has been known for some time that when certain kinds of extraneous substances are introduced into boiling fluids, considerable effect is produced upon the boiling point, vapour being formed either at lower points or with much increased facility. Thus Gay-Lussac has shewn that metal filings thrown into water, heated in a glass vessel, lowers the boiling point of the water 2° or 3° , and Mr. South pointed out the effect produced by putting platina wire or slips of platina foil into hot sulphuric acid, causing it to boil readily, quietly, and at lower points in glass vessels, than it otherwise would do, the difference here being several degrees.

Dr. Bostock has observed a remarkable fact of this kind in the extent to which the boiling point of ether may be changed by the introduction of a small chip of wood, or a portion of quill or feather of any kind. Ether, in a glass vessel, boiled freely at 112° , and with difficulty at 110° . Employing another glass vessel, it would not boil till the temperature had attained 150° , and the latter point was retained in other vessels. Repeating the experiment in a new vessel, it boiled earlier than before, but the vapour was observed to come off from one point where some substance had adhered to the glass. This led to the introduction of a small cedar chip, when the wood was quickly covered with bubbles, and the ether brought rapidly into ebullition. In this way ether boiled at 102° , which, without the wood, required 150° . The wood was not so effectual after some time as at first. When completely soaked with the ether it sunk to the bottom, and the ebullition nearly ceased; a fresh piece renewed it. Fragments of broken glass lowered the boiling point considerably. A small piece of metallic wire or copper filing, put into ether at 145° , caused a sudden and copious explosion of gas or vapour, and lowered the boiling point many degrees. Plunging a thermometer into the hot ether, caused production of bubbles at a temperature many degrees below the boiling point, no thermometer being present; after a time the effect ceased, but removal of the thermometer from the ether, and then re-immersion of it, produced a repetition of the effect. The cedar wood acted best when perfectly dry.

Alcohol of S. G. .848, boiled in a glass vessel at 182° , but by dropping in successive pieces of cedar wood the boiling point was reduced as much as 30° and 40° . The boiling point of water, Dr. Bostock found, was altered 4° or 5° by chips of cedar wood, requiring a temperature of about 217° when heated in a glass tube, by means of hot brine, but being brought down to the usual boiling point by the chips.—*Ann. Phil. N. S.* ix. 196.

2. *Oersted on Accelerating Distillation.*—In *Gehlen's Journal*, i. 277, I have related a few experiments which demonstrate that the disengagement of gas in a fluid resulting from chemical decomposition, never takes place, except in contact with some solid body. This principle may, without doubt, be applied to the disengagement of vapours. If a metallic wire be suspended in a boiling fluid, it instantly becomes covered with bubbles of vapour. Hence it might be concluded that a large number of metallic wires, introduced into a fluid which we wish to distil, would accelerate the formation of vapours. To prove this opinion, I introduced ten pounds of brass wire, of one-fifth of a line in diameter, loosely rolled up, into a distillatory vessel, containing 20 measures (about 10 pints) of brandy; the result was, that seven measures of brandy distilled over with a heat which, without the wire, was capable of sending over only four measures.

An expedient similar to this has been long in common use in England. When a steam-boiler has become incrustated with so much earthy matter that the contained water ceases to boil with rapidity, it is customary to throw in a quantity of the residue obtained from malt, by extracting its soluble portion, and which chiefly consists of small grains or fibres. Here the disengagement of vapour is promoted by the large number of thin and solid particles.—*Ann. Phil. N.S.* ix. 157.

The Editor of the *Annals* considers it probable that M. Oersted refers to the statement made by Mr. Bald in the *Edin. Phil. Jour.* ii. 340. That gentleman states that *cornings* are used for this purpose; they are the radicles of barley produced by malting, and separated before the malt is sent to market. About a bushel of these is thrown into the boiler, and when the steam is raised there is not only a plentiful supply to produce the full working speed of the engine, but an excess going waste at the safety valve. This singular effect will continue several days.

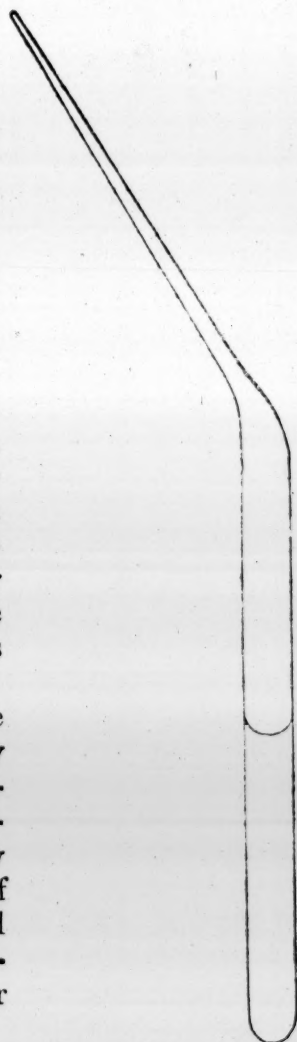
3. *Maximum Density of Water.*—Professor Hälloström, in a memoir which has appeared in the *Swedish Transactions* for 1823, deduces the temperature of the maximum density of water, as 39.394° Fahrenheit. Endeavours were made to estimate every cause which interfered with the experiments, such as dilatation of glass, &c., and he thinks the limits of uncertainty are 0.428° Fahrenheit on either side of the above number.

4. *On the substitution of Tubes for Bottles, in the preservation of certain Fluids, such as Chloride of Sulphur, Protochlorides of Phosphorus, and Carbon, &c.*—There are many fluids in the laboratory, which are much more conveniently retained in tubes, such as that depicted in the margin, than in bottles, and from which they may be taken in a less wasteful manner when required for the purpose of experiment. A piece of glass tube, a quarter of an inch or

more in diameter, being selected, is to be closed at one end by the blow-pipe; and then, being softened near the other end, is to be drawn out obliquely, so as to form the long narrow neck represented in the wood-cut, but to which, in the first case, the short piece of tube is to be left attached; this forms a funnel, into which the preparation to be preserved is to be put. Then, warming the body of the tube, the expanding air passes out through the fluid; and, afterwards, on cooling the vessel, the liquid descends into it. A small spirit-lamp flame being now applied at the upper part of the long neck, softens the glass, which is then to be drawn out to a fine point and sealed. In this state the substance may be preserved clean and pure for any length of time.

If a small portion be required for an experiment, the extreme point of the neck is to be opened by pinching it off, the tube is then to be inclined until the quantity required has entered the neck, where, by capillary attraction, it will form a small column, and the tube being warmed by the hand, the atmosphere within it will expand and expel the portion of fluid on to the place required. A very little practice will enable the experimenter to judge of the quantity he is forcing out, and in this way he may take a portion not larger than the 1-20th of a common drop, or he may take the whole contents of the tube. When the quantity required has been taken out, the tube is to be placed in an upright position, and the flame of a lamp, or candle, or even a piece of paper, closes the aperture in a moment and as perfectly as before.

I have found these tubes very serviceable when working with substance either very small in quantity or obtained with great difficulty, in consequence of the entire prevention of waste resulting from their use. They are easily labelled by scratching the name of the substance with a diamond on them, and may conveniently be retained by putting several of them together into a tumbler, or other glass of that kind.—M. F.



5. *Supports for Substances before the Blow-pipe.*—Lieutenant-colonel Totten has adopted a modification of Mr. Smithson's contrivance. He pulverises a portion of the mineral to be tried, forms a paste of it with very thick gum-water, and rolling it under the finger moulds it into an acute cone, sometimes nearly an inch

long, and 1-20th of an inch in diameter at the base. These may be directed accurately upon the minutest visible particle, and being slightly moistened at the point with saliva, the particle will adhere to the apex under the strongest blast of the blow-pipe. These cones need not be more than one-fourth or one-fifth of an inch in length, for so effectually is the conducting power of the mass interfered with by the pulverization of the mineral, that one of them, half an inch in length, may be held in the fingers whilst the apex is in the focus of heat. A great advantage of this method over others is, that if fusion ensues, it is owing entirely to the nature of the substance experimented upon, and not to the agency of foreign substances acting as fluxes.—*Ann. Lyceum, of New York.*

6. *Examination of Fused Charcoal.*—At last a specimen of fused charcoal or supposed artificial diamond has been examined. The specimen was obtained by Professor Macneven of New York, by means of Hare's deflagrator, was sent to Dr. Cooper, and by him presented to Mr. Vanuxem, who examined it, having always been very sceptical on the subject of the fusion of charcoal. It consisted of a large and small globule connected together by a thread, colour black, without lustre, opaque. When struck it yielded without breaking, receiving a polish like that of iron; when filed it gave way as iron or soft steel would do; it was attracted by the magnet, and when hammered was malleable. Nitric acid, when heated, acted violently on it, and, ultimately, peroxide of iron and a little silica were obtained. The proportion of silex to metallic iron was about 11 : 5.

Such, therefore, is the nature of the black fused charcoal, and there can be no doubt that the colourless fused charcoal is also due to the impurities of the charcoal acted upon, as was formerly supposed*.—*Philadelphia Journal.*

Messrs. Silliman and Hare deny, however, that Mr. Vanuxem has operated on a proper specimen.

7. *Selenium in Anglesea Pyrites.*—An account is given in the *Annals of Philosophy, New Series*, ix. 52, of the production of selenium, during a process in which sulphuric acid, made from Anglesea pyrites, is used. The acid is prepared by Mr. Mutrie of Manchester, and is used by Mr. Thomson, who observed the production of the selenium in the preparation of muriatic acid. The selenium distils over with the muriatic acid into receivers, and in the course of two or three days, falls down as a reddish brown substance. The proportionate quantity produced from the sulphuric acid appears to be very small.

This selenium has been examined by Mr. Children: a fragment,

* *Quarterly Journal of Science*, xvi. p. 157.

heated on platina foil by a spirit-lamp, tinged the flame of an azure blue colour. A portion heated in a glass tube, gave off at first acidulous water, then some sulphur, afterwards a yellow vapour, condensing into a red sublimate, arose, and the residuum, by being heated in a tube open at both ends, left a grey earthy substance, principally silica and lime, amounting to about 53 per cent. of the weight of the original substance; consequently, there was about 47 of volatile matter, of which by far the largest portion was the red sublimate. It had fused and spread over the inner surface of the tube. A portion of it gave the same blue colour to flame before-mentioned, but more intense. Another portion, in an open tube, sublimed without giving off any sulphur, exhaling a strong odour like that of horseradish. It fused when heated gently, remaining awhile in a pasty state. It has a metallic lustre, a deep brown colour by reflected light; conchoidal fracture with a vitreous lustre; easily scratched by a knife; brittle; powder deep red; adhering when rubbed in a mortar, having then a grey, smooth, and somewhat metallic lustre. When in very thin lamina it is transparent, being of a beautiful cinnabar red colour.

8. *Alloy of Antimony and Potassium, first produced by Geoffroy.*—M. Serullas observed, amongst numerous other facts, on the production of alloys of antimony and potassium, that if emetic tartar be heated to whiteness in a covered crucible for two or three hours, there will be obtained, when cold, a carbonaceous mass which inflames spontaneously on exposure to the atmosphere. M. Serullas states, that this pyrophoric property of antimony, heated with carbon and potash, was pointed out by Klaproth, who, without knowing the cause, had observed the effect; but M. Derheins has more lately shewn, that the same property had been observed by Geoffroy, in 1736, and described by him at length, in *l'Histoire de l'Academie*, &c., for that year. He calls it a new detonating phosphorus made with antimony. It was obtained by mixing one ounce of diaphoretic antimony with two ounces of black soap, putting the mixture by degrees into a hot crucible, ultimately adding another ounce of soap, covering up the vessel and heating it violently, it was left to cool. In the evening when opened, it suddenly took fire by contact of air, and burnt with explosion; there were no fluid scoria, but the whole had formed a spongy mass. The process was repeated several times and always with the same result.—*Journ. de Phar.* 1824. p. 631.

9. *Composition of Crystals of Sulphate of Soda.*—It is known that when a hot strong solution of sulphate of soda is put into a vessel and closed up, it may be reduced to common temperatures without crystallizing, although, if the vessel be opened, abundance of crystals will immediately form. It has also frequently been

observed that, in some circumstances, crystals would form in the solution during cooling, even though the vessel had not been opened or agitated. These crystals, when observed in the solution, are very transparent and of a large size; they are quadrangular prisms, with diedral summits. Upon opening the vessel, the surrounding solution crystallizes rapidly, enveloping the first formed set of crystals with others, which, however, are very readily distinguished from them in consequence of their immediately assuming a white opaque appearance. Upon taking out the crystals, those first formed are found to be much harder than the usual crystals of sulphate of soda, and, when broken, it is found that the opacity is not merely superficial, but that it penetrates them to a considerable depth, and even at times throughout.

These harder and peculiar crystals are readily obtained by closing up a solution of sulphate of soda, saturated at 180° , in a Florence flask, boiling the solution in the flask so as to expel the air before closing it. Upon standing 24 hours, fine groups of crystals are formed. When the flask is opened the solution deposits fresh crystals, but on breaking the flask, the latter may be scraped off by a knife in consequence of the superior hardness of the first set.

The hard crystals when separated are found to be efflorescent, like those of the usual kind, and they ultimately give off all their water, leaving only dry sulphate of soda. When a given weight was heated in a platina crucible, one half their weight passed off as water, the rest being dry salt. They, consequently, contain eight proportionals of water, or 72 sulphate of soda, and $8 \times 9 = 72$ water. The usual crystals of sulphate of soda contain 10 proportionals of water.

When crystallized sulphate of soda is heated in a flask, a part of it dissolves in the water present, whilst the rest is thrown down in an anhydrous state. The solution at 180° appears to contain one proportional of salt 72, and 18 proportionals of water 162; from which, if correct, it would result, that when the crystals are heated to 180° $\frac{5}{9}$ of the salt take all the water, whilst $\frac{4}{9}$ separate in the dry state.—M. F.

10. *Use of Chloride of Calcium, as a Manure.*—M. Chevalier finds that chloride of calcium is useful as a manure, only in the state of very diluted solution, for that, when applied in the solid state to the soil, it destroyed vegetation. It is something, however, to obtain a confirmation of the results mentioned, vol. xvii. p. 362. even though that confirmation be rather general.—*Jour. de Pharmacie*, 1824, p. 611.

11. *Separation of Strontia and Baryta.*—*Fluate of silica and baryta* precipitates in crystals almost insoluble. The *fluat* of silica

and strontia is very soluble in excess of acid. On this difference in properties, M. Berzelius founds a process for the separation of these two earths: he considers it an easy one and sufficiently exact for the estimation of their quantity. The mixture of the two earths is to be dissolved in muriatic acid, then solution of silicated fluoric acid is to be poured in, the baryta will precipitate, and its weight is determined by that of the precipitate; a very minute quantity of sulphuric acid added to the solution, will precipitate the rest of the baryta without acting on the strontia. The liquid is to be filtered: evaporated to dryness, and the residue decomposed by sulphuric acid.—*Ann. de Chimie.* xxvii. 301.

12. *On Combinations of Carbon and Iron, Pig Iron, &c.*—A long experimental memoir by M. Karsten, on the combination of iron and carbon, is contained in the *Annales des Mines*, ix. 657. We have not time at present to analyze it, but give the results as drawn up by the author, and appended to the memoir.

1. White cast iron and tempered steel contains the carbon combined with the whole mass of iron. 2. Lamellated white cast iron presents a perfect combination of iron with carbon; it always contains more carbon than the grey cast iron. 3. Iron and steel, not tempered, contain the carbon in the state of carburet. 4. Cold grey cast iron contains the larger part of its carbon, in the state of graphite and of mixture: this graphite contains no iron, but constitutes the carbon in all its purity. 5. The rest of the carbon contained in the grey cast iron, may be found either combined with the whole mass, or forming a definite carburet, which is afterwards dissolved in the metal, as is the case with soft iron, or steel. 6. All the varieties of carburetted iron, considered in the liquid state, contain the carbon dissolved in the mass of metal in indefinite proportions. 7. Finally. The graphite separates from the metal at the moment of congelation, and if there be other carburets of iron, they separate at a later period.

Remarking on the means generally proposed for the separation or estimation of the carbon in carburetted iron, M. Karsten finds grounds of objection to them all. That proposed by M. Vauquelin, namely, the use of sulphurous acid, he states to be uncertain and inaccurate from the formation of sulphuret of iron.

13. *Massive Copper obtained by the Moist Process.*—M. Clement has described the production of metallic copper from a solution, in a state as dense and compact as that afforded by fusion. It occurs in the manufactory of M. Mollerat during a process, the object of which, is to prepare sulphate of copper by the calcination of copper with sulphur. A solution of the sulphate is obtained turbid from the presence of insoluble sub-sulphate. This solution is introduced into a wooden tank, that it may become clear by deposition;

the tank is sunk partly into the ground, and it is upon its internal surface, and always where two staves meet, that the mushrooms of metallic copper form; they appear as small masses, enlarge by degrees, and ultimately are of considerable size; one weighed 75 grammes (1158 grains).

When detached from the wood, they are found on one side to be marked by the stries of the wood, whilst the other surface is mamillated, and presents very small, but brilliant crystalline facets. They are formed, in consequence of the presence of sulphate of protoxide of copper, in solution, which, in passing to the state of per-sulphate, deposits one portion of copper in the metallic state, whilst the other is peroxidized. The effect does not require access of air. M. Clement was particularly struck with the cohesion acquired by the copper thus precipitated from a solution; it was such as to allow of the metal being forged at common temperatures, and being reduced to thin leaves. Its specific gravity, also, was 8.78 being equal to that of fused copper. When filed, the surface produced was as brilliant and as close as that of an ingot of common copper.—*Ann. de Chim.* xxvii. 440.

At some of the Anglesea copper-mines, the solution of sulphate of copper pumped up by the engines is decomposed by the introduction of iron, and the copper is precipitated. It frequently happens there, that the circumstances are such as to produce copper as compact and dense as fused copper, and there is no difficulty in selecting such specimens from the pits in which the precipitation is usually performed.—*Ed.*

14. *Ammoniacal Chromate of Copper.*—M. Vuaflart has observed, that chromate of copper prepared by precipitating sulphate of copper by chromate of potash, and which is of a reddish brown colour, is soluble in diluted ammonia, producing a clear solution of a beautiful and deep green colour. When the solution is evaporated, the reddish chromate of copper appears as the ammonia flies off.

This solution was made for the purpose of decorating the front of a druggist's shop. The green is finer than most of those obtained in the usual manner, and undergoes no change by length of time or exposure to bright light. It is readily prepared by adding solution of chromate of potash to ammoniacal sulphate of copper.—*Jour. de Phar.* 1824. p. 607.

15. *Artificial Crystals of Chromate of Lead.*—A diluted solution of nitrate of lead being added to a very alkaline solution of chromate of potash, and left at rest for some time; there was found in the mixed solution small red crystals, which, upon examination, proved to resemble, in all their characters, the native chromate of lead from Siberia.—M. F.

16. *Compound of Muriate and Hydrosulphuretted Oxide of Antimony.*—Sulphuretted hydrogen added to solution of muriate of antimony, throws down a yellow coloured precipitate, generally regarded as a hydrosulphuretted oxide of antimony, but which is a compound of that substance with neutral muriate of antimony. Heat separates the latter body, and sulphuret of antimony remains. A similar effect is produced by exposing the precipitate in a close vessel to the solar light.—*Gmelin. Ann. Phil. N. S.*

17. *New Mineral. Titaniferous Cerite.*—M. Laugier has lately analyzed a mineral from the Coromandel coast, which, from its composition appears to be peculiar and distinct. It was brought to Europe by M. Leschenault de la Tour. It was an irregular mass, of a blackish-brown colour, a vitreous conchoidal fracture, hardness equal to that of the Gadolinite, to which mineral it had some analogy, but differing from it by swelling up when heated. It lost only 1.25 per cent when heated, although it contained $\frac{1}{10}$ of water; a cause for this effect will be evident presently.

Acids and alkalis both act upon it, and M. Laugier employed these agents in his analytical experiments. He found it to yield 36 oxide of cerium; 19 oxide of iron; 8 lime; 6 alumine; 11 water; 1.8 oxide of manganese; 19 silica; 8 oxide of titanium. These quantities surpass the 100 used by 9.55 parts; this is occasioned by the protoxide of cerium which exists in the mineral becoming peroxide during the process, and this is also given as the reason why so little loss of weight occurs when the substance is calcined.

M. Laugier remarks, that this mineral is analogous in its composition to the substances distinguished by Berzelius and Hisinger as Orthite, Allanite, and Cerine; that it particularly resembles *orthite*, especially in its physical characters, but differs in the presence of titanium. It may therefore be regarded as a new variety of *titaniferous cerite*.—*Ann. de Chimie. xxvii. 313.*

18. *On Chloride of Titanium.*—Having the opportunity of examining the foundation of a blown out furnace, at the Low Moor Iron Works, Mr. E. S. George removed from it a quantity of the stone work, penetrated completely by metallic iron, sulphuret of iron, carbonaceous matter, and titanium in brilliant cubes. Part of this mixture, acted upon by muriatic acid, evolved hydrogen, and sulphuretted hydrogen, the iron and earths were dissolved, and a mixture of titanium cubes and grains of sand was left; the silex was easily removed.

Sixty grains of the metallic titanium were placed in a glass tube and dry chlorine passed over; no action was perceptible until the titanium was heated to redness, but then a fluid gradually condensed in the cool part of the tube, and was collected by in-

clining the apparatus. The fluid was transparent, colourless, possessing considerable density, evolving dense fumes in the atmosphere, having a pungent odour resembling that of chlorine: the fumes depend upon the presence of moisture: it boils a little above 212° , and condenses without further change. A drop of water added to a few drops of the liquid caused rapid disengagement of chlorine, much heat, and the production of a solid salt. This salt is deliquescent, soluble in water, the solution having all the properties of muriate of titanium. It yields a brownish-red precipitate with prussiate of potash, a dark red with infusion of galls, with pure potash a gelatinous precipitate, soluble in excess of muriatic acid; ammonia throws down a white precipitate. When the chlorine is not dry, the same salt crystallizes in the tube. Into a long test tube, 14.6 grains of the fluid were introduced, and afterwards a weighed portion of water added very gradually; chlorine was rapidly disengaged, and heat produced. When cold, the loss was found to be four grains, and the solution gave a dark-red precipitate with gallic acid. The fluid, therefore, is a perchloride of titanium, which, by losing chlorine, becomes a protochloride, and that by solution in water a muriate.

Water was added to a solution of the muriate formed by the decomposition of the perchloride by water, and the solution divided into two equal parts: the one decomposed by potash gave 7 grains oxide of titanium; the other, by nitrate of silver, gave 15 grains chloride of silver = 3.6 chlorine. The muriate, therefore, contains oxide of titanium, 7; muriatic acid, 3.74. Supposing the muriate to contain one atom acid, and one atom oxide, the latter will be the protoxide, and the weight of titanium will be 61.2. Probably, the true number is 64, as indicated by Mr. Rose's experiments.

Muriate.	{	Oxide of Titanium . . .	7.00
		Muriatic acid . . .	3.74
Protochloride.	{	Titanium	6.12
		Chlorine	3.64
Perchloride.	{	Titanium	6.66
		Chlorine	7.94

Ann. Phil. N. S. ix. 18.

19. *Peschier on Titanium in Mica.*—In consequence of the results obtained and published by M. Vauquelin*, with regard to the presence and the quantity of titanium in various specimens of mica, M. Peschier, to whose results those of M. Vauquelin's are opposed, has published the process by which he separates

* *Quarterly Journal of Science*, vol. xviii. p. 392.

titanium from other substances, in minerals containing it, and which he thinks requisite to its correct estimation.

1. The mineral is to be finely pulverised, heated with two parts of potash, the crucible being taken from the fire when incandescent; the produce is to be diffused through water, put on to a filter, and washed, until test paper ceases to be affected. The washings are slightly supersaturated, evaporated to a moist solid state, diffused through water, and put on to a filter: the silica separated when washed and dried, is treated with oxalic or muriatic acid, and the washings added to the former washings; infusion of galls is then added, and the solution made slightly alkaline and concentrated; if the characteristic brown red tint of titanium appears, the solution is set aside, to be subjected to further examination.

2. The residue, insoluble in potash, is boiled with a mixture of one muriatic acid and six or eight of water; if a larger quantity of insoluble matter than was expected appears, it is treated with potash, as before. The acid solutions are saturated with an alkaline subcarbonate, and after having separated the precipitate, the liquid is examined, as was that in section one, and if it contains titanium, is mixed with it.

3. The precipitate formed in the last section, is exposed to the action of potassa, and as the titanium is partly dissolved in the alkali with the alumina, and partly thrown down again with it, by muriate of ammonia, sulphate of ammonia is used instead, which is found to precipitate only alumina; when this earth has been received and washed on a filter, the liquids are evaporated to a moist saline state, and by solution in water, such portion of silica separated from it as was previously dissolved. Infusion of galls is added to the washings, all of which are added to the preceding washings of 1 and 2.

4. As the titanium does not dissolve in potash so readily as the alumina which accompanies it, the insoluble portion preserves a gelatinous appearance; to separate the titanium, the residue is dissolved in muriatic acid, the silica removed by a filter, the iron separated by ferroproussiate of potash, the liquid saturated by an alkaline subcarbonate, and boiled. The precipitate which forms, is white, bulky, and resembles alumina in appearance; it may be composed of oxide of titanium, magnesia, and lime; a strong heat renders the first insoluble in acids, and consequently, the earths may be removed by digestion of the whole for some hours in weak acid, as distilled vinegar. The insoluble portions are to be separated on a filter, the liquid treated with ammonia for the magnesia, and with oxalate of ammonia for the lime; the operation has been well performed, if infusion of galls produces no effect in the remaining solution.

5. As titanium forms double salts with all the acids, and as its

tannate is readily dissolved by infusion of galls, the portion of titanium which otherwise would be lost, from the influence of these two causes, may be obtained by evaporating to dryness all the solutions which have been set aside, heating the residue to redness, dissolving in water, filtering, washing the insoluble portion, again heating it, to burn off carbonaceous matter, then washing it with weak acid, and it will be the titanium required. If contaminated by iron or manganese, they may be removed by digestion in nitromuriatic acid, and repeating this process twice more on the washings, adding each time infusion of galls, all the titanium may be obtained from the mineral analyzed.

In consequence of the tendency of titanium to form double salts, I have always separated several grains from the salts obtained in the search after the alkaline principle of this class of minerals; its presence is known by the spongy state of the chlorides of potassium or sodium, which, when freed from ammonia, and heated to redness, will not enter into fusion, and which require several solutions, evaporations, and calcinations, for its complete separation.

Such are the minute operations which M. Peschier has found indispensably necessary in the analysis of minerals with a base of titanium (the number of which is greater than is supposed), and by the aid of which, the foliated black mica of Siberia gave—Silica, 24; Alumina, 8.5; Magnesia, 5; Peroxide of iron, 30; Manganese, 0.7; Titanium, 21; Potash, 5.7; loss by fire, 2.75; total 97.65. Talcs, Chlorites, and Steatites, have yielded from 19 to 30 per cent. of a substance which, like that from mica, which M. Peschier has called titanium, and the titanium obtained from rutilite, forms a gelatinous transparent yellowish mass by evaporation at a gentle heat, from its muriatic solution; furnishes, like it, a very voluminous gelatinous and white precipitate, by the saturation of its acid solution; which yields a yellowish precipitate by infusion of galls, deepening in colour by a slight supersaturation of the acid, becoming brown, and dissolving by further addition of the re-agent, producing a blood-red solution; is soluble in pure alkali; forms double salts with all the acids; becomes insoluble in acids by a strong heat, and consequently, possesses all the characters of titanium, with this slight difference, that its precipitate by infusion of galls is not quite so abundant or so deep in colour, nor does it always become of a brown colour by heat; these differences are, however, considered as of but little importance, compared with the many positive characters it possesses.—*Ann. de Chimie.* xxvii. 281.

20. *Wöhler on a Compound of Cyanuret and nitrate of Silver.*—Concentrated solutions of cyanuret of mercury and nitrate of silver, being mixed together, no precipitate fell, but after a few minutes,

small white crystals formed, which were repeatedly washed with water, and dried. These, heated above 212° , first fuse, then boil up, and detonate vehemently with a cracking noise, and a purplish flame, resembling that of cyanogen. The residue is cyanuret of silver, which, by continued ignition in the air, becomes metallic silver. In close vessels, mercury sublimes during the experiment. Muriatic acid disengages hydrocyanic acid from the crystals which, driven off by heat, is succeeded by strong odour of chlorine; on evaporation chlorides of silver and mercury remain. Solution of the crystals mixed with muriate of baryta, filtered and evaporated, yields a saline mass, containing octoedral crystals of nitrate of baryta. Alcohol also extracts from it cyanuret of mercury; consequently, the original crystals are a compound of cyanuret of mercury and nitrate of silver.

This substance is difficultly soluble in cold, but plentifully in hot water; it crystallizes in large transparent prisms, like those of nitre. Alcohol dissolves it as much as water does. It is soluble in boiling hot nitric acid, without decomposition. Alkalies precipitate cyanuret of silver from its solution, mixed also with sub-nitrate of mercury. Repeated solution in pure water effects a similar decomposition, to a slight extent. Heated below 212° they lose water, and become opaque, without altering in form; 100 parts gave off 7.6 of water.

To determine the quantity of silver in the compound, one gramme was treated with excess of muriatic acid, carefully evaporated to dryness, and the corrosive sublimate expelled by heat, 0.32 of a gramme of fused chloride of silver remained, equal to 0.2588 of oxide of silver, or 37.96 per cent. of nitrate of silver. The cyanuret of mercury was ascertained by dissolving 0.67 of the crystals in hot water, precipitating the crystals by (hydro?) cyanic acid, filtering, and evaporating to dryness, to expel the excess of acid and the nitric acid, 0.36 of pure cyanuret of mercury remained = 53.74 per cent. Hence, 100 parts consist of

Nitrate of silver	. . .	37.96	— 1 atom
Cyanuret of mercury	. . .	53.74	— 2 —
Water		7.60	— 8 —

This compound must be regarded as a true saline substance, in which nitrate of silver acts as acid, and cyanuret of mercury as base; and the existence of water in it, a substance possessed by neither of its elements in a separate state, affords additional reason for ranking it among salts.

M. Wöhler then endeavoured to form other similar compounds of nitrate of silver with metallic cyanurets. Newly precipitated cyanuret of silver, boiled in solution of nitrate of silver, dissolved slowly but completely. As the temperature fell below the boiling point, there were deposited white acicular crystals in

such quantity, that the liquid became a magma. These were dried upon bibulous paper; they would not bear washing, the affinity by which the compound was formed being so weak that contact with water resolved it into pulverulent cyanuret of silver, and solution of the nitrate, so that strong solution of nitrate of silver is required in its preparation. When heated it fuses, detonates, and leaves cyanuret of silver; it contains no water. If analogous to the preceding salt, it should contain

Nitrate of Silver	. . .	38.79	— 1 atom
Cyanuret of Silver	. . .	61.21	— 2 —
		100	

Equal to 70.76 per cent metallic silver. This statement was confirmed by the results of an experiment; 0.43 of the compound decomposed by muriatic acid giving 0.387 chloride of silver, or 69.74 per cent. metallic silver.

All attempts to form other similar compounds failed, the other metallic cyanurets decomposing the nitrate of silver used in the experiment.—*Ann. Phil. N. S.* ix. 131.

21. *Sulphates of Cinchonia and Quinia.*—The following results and remarks are abstracted from a memoir by M. Baup.—*Ann. de Chimie*, xxvii. 323.

Super Sulphate of Cinchonia.—Prepared by adding very pure sulphuric acid to the neutral sulphate, evaporating until a pellicle forms, and setting it aside to crystallize. If the crystals are small and fragile they should be re-crystallized; if not soluble in their weight of cold water, it is a proof that neutral sulphate is present, when more acid must be added. This salt is colourless, unaltered at common temperatures in the air, but readily efflorescing if the temperature be slightly raised; crystals are rhomboidal octoedra, always imperfect, and readily cleaving perpendicularly to the larger axis. It is soluble in 0.46 of water at 57°.2, or in 0.9 of alcohol, specific gravity 0.85. Ether does not dissolve it.

Neutral Sulphate of Cinchonia.—Crystals short rhomboidal prisms of 83° and 97° cleaving parallel to the planes of the prism, soluble in 6.5 of alcohol, specific gravity 0.85 at 55°.4, or in 11.5 of absolute alcohol, or in 54 of water.

The elements of these salts were estimated as follows:—The water from the loss occasioned by exposure in a stove, at a temperature of 248° Fahr. until no further diminution took place; the sulphuric acid by precipitation from a solution of the sulphate in diluted acetic acid, by muriate of baryta; the cinchonia by the deficiency. From these experiments it was deduced that 39 would

represent the number of cinchonia, oxygen being 1 *, and that the neutral sulphate contained

		Crystallized		Dry
1 atom cinchonia	39	84.324	. . .	88.636
1 — sulphuric acid	5	10.811	. . .	11.364
2 — water	2.25	4.865		100.000
		100.000		

Super Sulphate of Cinchonia.

		Crystallized		Dry
1 atom cinchonia	39	67.241	. . .	79.592
2 — sulphuric acid	10	17.241	. . .	20.408
8 — water	9	15.518	. . .	100.000
		100.000		

Neutral Sulphate of Quinia.—This salt readily effloresces in the air, three-fourths of the water passing off, and one-fourth remaining. Soluble in 740 of water at 55.4° Fahr., and in 30 parts at 212°. Alcohol of specific gravity 0.85 dissolves $\frac{1}{80}$ at common temperature; in much greater proportion at the boiling heat.

Super Sulphate of Quinia.—When pure, colourless, unaltered in the air at common temperatures: crystallizes in rectangular prisms, cleaving parallel to the planes of the prism. The crystals obtained by diminished temperature are small and acicular, but spontaneous evaporation yields them of a large size. It is soluble in 11 of water, at 5° Fahr.; in 8 parts, at 71° Fahr.; and in its water of crystallization at 212° Fahr. It is soluble in alcohol, and the crystals obtained from solution in absolute alcohol immediately fall into powder in the air.

Some of the crystals still wet were put upon filtering paper, in a cold damp place: when dry they were rapidly weighed, and then left to effloresce at a temperature of 68°. In this way it lost three-fourths of its water, and the other fourth also when dried in a stove. The dry sulphate exposed to the air resumed the fourth it had lost in the stove.

The experiments of M. Baup led him to adopt the number 45 for quinia. The composition of its sulphates are according to him as follows:

Super Sulphate			Crystallized		Dry
1 atom quinia	45		61.644	. . .	81.819
2 — sulphuric acid	10		13.698	. . .	18.181
16 — water	18		24.658		100.000
			100.000		

Neutral Sulphate			Crystallized		Dry
1 atom quinia	45		76.272	. . .	90
1 — sulphuric acid	5		8.474	. . .	10
8 — water	9		15.254		100
			100.000		

* Cinchonia 312, hydrogen being 1. Quinia, 360.

Neutral Sulphate effloresced

1 atom quinia	45		86.12
1 — sulphuric acid	5		9.57
2 — water	2.25		4.31
			100.00

M. Baup also remarks that in medicine the same doses of neutral sulphate of quinia, which after having been merely dried in a cool damp place is preserved in a well-closed bottle, or of the same sulphate kept in a badly-closed bottle or in the air, should in no case be employed indifferently. In the first case the salt may contain only 76 per cent of quinia, whilst in the latter it may rise as high as 86 per cent. He considers, that constancy would be most readily ensured by the use of the effloresced salt, as it is always of definite and invariable composition.—*Ann. de Chim.*, xxvii. 323.

22. *Parilline, or salifiable Base of Sarsaparilla*.—A new vegeto-alkaline base is said to have been discovered by M. Galileo-Palotta, in sarsaparilla. The process for obtaining it is as follows: The sarsaparilla is to be cut, crushed in a mortar, six times its weight of common boiling water poured on to it, and the infusion to be continued for eight hours in a closed vessel; the liquor is then to be passed through a cloth, an equal quantity of water poured on to the residue, and treated as the first portion. The infusions added together have a deep amber-colour, and are slightly bitter and nauseous. Sufficient milk of lime is to be added, until the mixture sensibly reddens turmeric paper, agitating the liquid strongly at the same time with a wooden spatula. The infusion changes colour and becomes brown, and ultimately a grey pulverulent substance precipitates, which is to be collected on a fine cloth, and whilst moist mixed with water saturated with carbonic acid; it is then to be dried in the sun, and reduced to a fine powder. This powder is to be boiled in a matrass for two hours with alcohol, specific gravity .817; filtered, and the residue redigested in fresh alcohol.

The alcoholic solutions being mixed, are to be distilled in a glass retort by a water-bath, until the liquid in the retort becomes sensibly turbid, when it is to be put into a capsule and left at rest: in a short time a white pulverulent substance precipitates, and attaches itself to the surface of the vessel. The liquid is to be poured off, the vessel put into a stove heated to 25° R (88° Fahr.) When sufficiently dry, it is to be collected and preserved in close vessels, being *Parilline*. The liquid decanted and evaporated slowly to dryness, yields a solid compact mass, slightly deliquescent, and of a dull colour. It is parilline combined with a particular colouring matter, but may be purified readily by the usual means.

Parilline is white, pulverulent, light, unaltered in the air, bitter, austere, slightly astringent and nauseous, and having a particular odour. It is heavier than distilled water. Insoluble in cold water; very little soluble in hot water. Slightly soluble in cold alcohol; soluble in hot alcohol. Impure parilline is insoluble in cold water, soluble in hot water, and in concentrated hot and cold alcohol.

This substance slightly reddens turmeric paper. Heated to 212° it fuses, becomes black, is partly decomposed: at a higher temperature it decomposes like vegetable substances containing no nitrogen. Concentrated sulphuric acid decomposes it: diluted sulphuric acid is neutralized by it, forming a sulphate. All the acids unite to it, forming salts.

After certain experiments made upon himself, in which M. Palotta took at different times: from 2 to 13 grains of pure parilline at once, he concludes that the substance is a powerful debilitating medicine, or one generally diminishing the vital energy; and, that in proportion to the dose given: to this property it also unites irritating powers.

M. Planche states that he has repeated the process of M. Palotta, but has not as yet verified all the properties said to belong to the substance.—*Jour. de Phar.* 1824, p. 543.

23. *Experiments on Civet.* By M. Boutron-Charlard.—The experiments of which the following are an abstract, were made with an unexceptionably good and unaltered specimen of civet. It was a semi-fluid mass, unctuous, of a yellowish colour, becoming brown by time, thickening by contact with air, of a very strong and disagreeable odour when in quantity, but agreeable when weakened. A portion of it put into a close vessel in which also was placed a piece of reddened litmus paper, gave out ammonia enough in 24 hours to restore the blue colour of the paper: when distilled at a low temperature, a few drops of an ammoniacal solution came over. Digested with boiling ether a portion was dissolved, which, when obtained by evaporation, proved to be a mixture of elaine and stearine. The part insoluble in ether was soluble in hot solution of potash, with the exception of a few hairs and extraneous matter: the addition of nitric acid precipitated flocculi, of a substance which when collected burnt like animal matter. Hot alcohol after some time entirely dissolved the pure part of civet; on cooling, stearine was deposited, the remaining solution became turbid when dropped into water; evaporated it left an orange-yellow semi-fluid substance, which, by diluted muriatic acid and heat, was separated into resin and fatty matter.

Civet distilled with water yielded a few drops of volatile oil, of a biting hot taste, and having the odour of civet. The residue

gave an aqueous solution which evaporated to dryness, and freed from resin and extract by alcohol, left a peculiar yellow colouring matter, soluble in water, and combining with various bases. When incinerated, civet gave a voluminous coal, alkaline, and containing carbonate and sulphate of potash, phosphate of lime, and oxide of iron. Hence civet appears to contain, free ammonia, stearine, elaine, mucus, resin, volatile oil, yellow colouring substance, salts, &c. No benzoic acid could be found in it.—*Jour. de Phar.* 1824. p. 537.

24. *Composition of Common and Deoxidized Indigo.*—Mr. Dalton, in a paper published in the *Manchester Memoirs*, states, that in various experiments made at different times, but with very similar results, he finds that the quantity of oxygen required to convert the green or deoxidized indigo solutions in lime water into blue indigo is about $\frac{1}{7}$ or $\frac{1}{8}$ of the weight of the resulting indigo; and on the supposition that an atom of oxygen was added to one of indigo, concluded that the atom of indigo must weigh about 55 or 56. When indigo is destroyed by the oxymuriate, or rather, chloride of lime, as in the process adopted by Mr. Dalton for testing the value of indigo, he is persuaded from his experiments, that twice the quantity of oxygen is necessary that is required to revive it from the lime solution.

25. *Decolouring power of different substances.*—From certain experiments made by M. Payen on the decolouring power of various carbonized, mineral and other substances, it results that the schists of Mermet in Auvergne, and the carbon of calcined bones decolour in the highest degree. The schist removed $\frac{3}{5}$ of the colouring matter, the carbon $\frac{2}{3}$.—*Ann. de Chim.* xxvii. 360.

26. *Prize questions.*—By the Royal Academy of Sciences of Toulouse, for 1826, "A physico-mathematical theory of lifting and forcing pumps, demonstrating the ratio between the moving power employed and the quantity of water raised, the height to which it is raised being known, and all the obstructions which the force would have to overcome being considered." The prize a gold medal of 1000 francs value.

For 1827 "The manner in which the known anti-fermentative and anti-putrescent re-agents, such as camphor, garlic, peroxide, and perchloride of mercury, sulphurous acid gas, &c., oppose the spontaneous decomposition of vegetable and animal substances, and thus prevent the formation of alcohol in the former, and development of ammonia in the latter." A medal 500 francs in value.

III. NATURAL HISTORY, &c.

1. *Antiquity of Trees.*—In “Major Rooke’s Sketch of the Forest of Sherwood” it is stated, that in cutting down some timber in Berkland and Bilhaugh, *letters* have been found cut or stamped in the body of the trees, denoting the king’s reign in which they were marked. It seems that the bark was cut off, and the letters cut in, after which the next year’s wood grew over it, but without adhering where the bark had been cut. The ciphers are of James I., of William and Mary, and one of King John! One of those with James’s cipher was about one foot within the tree, and one foot from the centre: it was cut down in 1786. The tree must have been two feet in diameter, or two yards in circumference, when the mark was cut. A tree of this size is generally estimated at 120 years growth, which number subtracted from the middle year of James’s reign, would make 1492 the date of the planting of the tree. The tree with William and Mary had the mark about nine inches within the tree, and three feet three inches from the centre; cut down also in 1786. The mark of John was eighteen inches within the tree, and something more than a foot from the centre: it was cut down in 1791; but the middle year of John’s reign was 1207, from which, if we subtract 120, the number of years requisite for a tree of two feet diameter to arrive at that growth, it will make the date of its planting 1085, or about twenty years after the conquest. The tree, therefore, when cut down in 1791, must have been 706 years old; a fact scarcely credible; for it appears from the trees whose marks are better authenticated, that those exactly of the same size, when marked, had increased twelve inches in diameter in 173 years, whilst this tree had increased no more than eighteen inches in 584 years. Major Rooke says that several trees with this mark had been cut down, so that deception or mistake is scarcely possible.—*N. M. Mag.*

2. *Red Snow of the Alps.*—The notice, of which the following is part, was read to the Society of Natural History of Geneva, by M. Peschier: “I received in September, from M. Barras, Canon of the Convent of St. Bernard, a small bottle of water collected from the melting of this snow. The note accompanying it stated, that the spots of red snow assumed a deeper tint as the season advanced; that the portion from whence the water was procured had a coffee colour on its surface, but on removing two inches in depth, had a red colour. A deposit of the colour of moist earth occupied the bottom of the bottle, but on inclining it, it was found that the deposit reflected a red tint, like that of the snow; and having, in company with MM. de Candolle and Prevost, examined

it microscopically, we found that the red tint was due to the presence of small spherical globules of a bright red colour, which were surrounded by a gelatinous membrane, transparent, slightly yellow, the size of which varied from three to six millimetres in apparent diameter: in certain cases they were arranged in lines representing fibres; and they were mixed with fragments of moss and dust detached from the rocks.

A comparative observation was made on the deposit from the water of the red snow of the north, brought by Captain Ross, of which M. de Candolle possessed a small quantity; and it was found that the globules in it were identical with those of the Alp snow; so that these spots must be due to the developement of this kind of plant. M. de Candolle, who has studied them closely, does not view them as belonging to the *Uredo*, but rather as forming a new genus.—*Bib. Univ.* xxvii. 132.

3. *Artificial Production of Pearls.*—Mr. Gray whilst examining a specimen of the shell *Barbala Plicata*, in the British Museum, observed on it several very fine regular shaped semi-orbicular pearls of most beautiful water, and afterwards on examining the collection of pearls at the same place, he had an opportunity of observing in one of them attached to a fragment of the same shell, and cracked across, that it was formed of a thick coat consisting of several concentric plates formed over a piece of mother-of-pearl roughly filed into a plano-convex form like the top of a mother-of-pearl button: the other pearls all appeared to be formed in the same manner, and on some pieces of shell where the pearl had been destroyed or cut out, there was left a circular cavity with a flat base about the depth, or rather less, than the thickness of the coat which covered the pearls; proving that the pieces of mother-of-pearl had been introduced when the shell was younger and thinner, and that they must have been introduced between the leaf of the mantle and the internal coat of the shell.

Hence Mr. Gray was induced to expect that pearls of a very beautiful appearance and form for setting might be obtained with facility at home. He introduced similar pieces of mother-of-pearl into the shell of the *Anodonta Cygneus* and *Unio Pictorum*; this was done without much difficulty, the valves of the shell being forced open to a moderate breadth, retained so by a stop, the mantle slightly turned down, and the pieces introduced to some little distance by a stick; the stop was then withdrawn, and the animals returned to their natural habitation: of the thirty or forty pieces introduced, only two were pushed out again, the rest being placed by the animal in a convenient situation. In several afterwards destroyed, they were found near the posterior slope of the shell, where the pearls are situated in the *barbala*.

This plan of forcing the production of pearls by fresh water

bivalves, Mr. Gray thinks is the invention of the Chinese. On cutting out the pearls, it would be necessary that the shell should be cut through, so that the mother-of-pearl button may be preserved in its place, for if the back were removed, as it would be, were not the shell cut through, the basis would fall out, and then the pearl would be brittle.—*Ann. Phil. N. S.* ix. 27.

4. *Permanency of Human Hair.*—M. Pictet has lately made a comparison between a recent human hair and those from the head of a mummy from the Isle of Teneriffe; with respect to the constancy of those properties which render hair important as a hygrometric substance. For this purpose hygrometers, constructed according to Saussure's principles, were made, one with a recent hair, and the other with hair from the mummy. The ancient hairs were not so strong as the other, or of sufficient length alone, but the latter objection was obviated by tying four together. The results of the experiments were, that in both instruments, the interval between extremes of moisture and the dryness of the chamber (about 25°) was passed in three minutes: that the indications, like those of the thermometer, &c., were rapid on leaving the first term, and became slower on approaching the second: that the hygrometric quality of the Guanche hair is sensibly the same as that of the recent hair.—*Bib. Univ.* xxvii. 120.

5. *M. Peschier on the Cure of the Goitre.*—Being frequently called upon to administer aid in cases of goitre, M. Peschier, in 1816, endeavoured to separate from burnt sponge the substance which conferred on it useful properties; and thinking this might be the alkali, was induced to administer solution of sub-carbonate of soda, more or less disguised by other substances. This attempt was accompanied by complete success which has never yet been falsified. The effect was such, that M. Peschier says, at Aubonne (Canton de Vaud), and the neighbouring places, the remedy soon acquired a considerable name for its power of dispelling, or considerably diminishing, the goitre; and he refers to the evidence which may be abundantly obtained there, for confirmation. One or two cases are, however, quoted. Jan. 1. A young girl (Aubonne Isaline Cretigny), 14 years of age, well formed for her age, was brought to him for assistance. She had a goitre large enough to give the neck the appearance of a cylinder of the diameter of the head. Sub-carbonate of soda was administered in the proportion of two gros (118 grains) each day. At the end of the 20th day the goitre was so far diminished, and the girl's appearance so much altered, that she could scarcely be known for the same person. This was a particularly favourable case.

In ordinary cases, when the goitre is not connected with any general or constitutional affection, M. Peschier dissolves from two gros to half an ounce of sub-carbonate of soda in eight ounces

of water, and directs that a table spoonful of this solution should be taken twice a day in half a glass of wine, or sugar and water. Persons who have had no objection have taken the pure solution.

In cases where the enlargement of the thyroid gland has been accompanied by the same affection of the lymphatic ganglions of the neck, bitter and tonic roots, such as gentian, &c., have been added to the alkali; and also purgatives administered, such as rhubarb and senna, with anise or fennel seeds, the whole infused in a bottle of good wine, of which a quarter of a glass has been taken two or three times a day.

In one case, among many others, relief was afforded to a young person who had several enlarged glands on each side of the neck, even after it had been proposed to extirpate them by a surgical operation, the remedy being continued for several months. In other cases, very old suppurations of the glands have been corrected and cured, after they had resisted various modes of treatment.

When, in 1820, Dr. Coindet proposed the use of iodine, M. Peschier also applied it, but, except in one instance, always with the solution of soda. In one case, where tincture of iodine alone was used, the disease resisted the medicine for six weeks, and, at the end of that time, had become hard, producing a sensation of strangulation. Leaving off the use of iodine, Dr. Peschier first gave purgatives, and then alkali, and attained the end required.

From that time M. Peschier says he has resumed the exclusive use of sub-carbonate of soda, and always with success. He suggests the propriety of observing, whether the inhabitants of those places where the water is slightly alkaline are not less liable to goitre than in other places; and whether mixing habitually a small quantity of soda in the water intended to be drunk, would not entirely prevent the occurrence of this disease in places where it is now most readily manifested.—*Bib. Univ.* xxvii. 146.

6. *On Digestion in Ruminating Animals*, by MM. Prevost and Royer.—The experiments of these philosophers were made on sheep, the stomach of ruminating animals offering many facilities in the examination of the phenomena of digestion in consequence of its division into four parts.

The masticated food, moistened with saliva in the mouth, passes by the œsophagus into the paunch (*herbier*), a large cavity, occupying the greater part of the left side of the abdomen; its internal surface is provided with papillæ, formed from the mamellated tunic, which are covered with an epidermis, readily separating into plates and shreds. The paunch communicates by a large aperture, with the second division placed to the right of the œsophagus: the mamellated tunic here presents folds which project considerably, and circumscribed polygons, the areas of which are covered with fine papillæ. The food in this division is less solid than that

in the paunch ; returned to the mouth many times during rumination, it finally forms a paste, which passes directly from the oesophagus to the third stomach, by means of a groove, proceeding from the cardiac aperture of the paunch to the upper orifice of the third stomach.

The contents of the first and second divisions are similar to each other: the triturated mass which they contain is sensibly alkaline, probably because of the unsaturated soda of the saliva, and of the secretions of the two first stomachs. These were pressed together, and furnished a liquid and a hard residue. The liquid, boiled to separate the albumen, was then very carefully evaporated to dryness. The extract being put into warm water, left an insoluble portion of coagulated albumen, and by filtration a liquid was obtained, which, when evaporated, had on its surface a pellicle formed, which dissolved when stirred as jelly would have done. When cold the solution gelatinized, and upon being dried, left a brown substance, having a vitreous fracture, and slightly transparent. This substance had many characters of gelatine ; it was insoluble in alcohol and ether, soluble in cold water, more soluble in hot water. Mineral acids or corrosive sublimate did not precipitate it when cold, but boiled with the latter, flocculi formed, insoluble, and the liquid lost its power of gelatinizing. The portion of residuum insoluble in water was coagulated albumen, containing a little mucus, which could be separated by acidulated water, and then obtained by evaporation. These experiments with others lead MM. Prevost and Royer to conclude that the nourishing parts of the alimentary matter are, 1. Albumen of the vegetables extracted and retained in solution by the alkaline fluids proper to the animal ; and 2. Jelly, of which the properties have been mentioned, containing a certain quantity of mucus. The following result will give a general idea of the quantities concerned.

Alimentary portion of the two first stomachs	5.231 kil.
Liquid obtained by expression	2.753
Residue of the expression	2.478
There was obtained from the liquid,	
Dried jelly	16.78 grammes.
Dried albumen and mucus	27.52
And from the residue of the expression,	
Dried jelly	8.10
Dried albumen and mucus	4.82

The albumen and jelly had been washed with alcohol, to free them from chlorophyle and salts.

The third stomach has its cavity filled by numerous folds of the mamellated membrane: these are thin, large, and placed one against another like the leaves of a book. These strongly compress the food; the liquid it contains is separated, and runs into

the fourth stomach, which like the last is on the right of the paunch. It is larger in size, and communicates at the lower part with the duodenum, by an aperture corresponding to the pylorus of single stomachs; a very delicate mucous membrane covers it internally, presenting large valves, disposed in a longitudinal direction. The liquids which come into it from the third stomach undergo a remarkable change, from alkaline becoming acid, and a white opalescent flocculent matter precipitates on the valves, to which it adheres as a false membrane would do. This precipitate is the chyme, it is nearly pure albumen, and contains globules; it does not dissolve either in cold or hot water, or in mineral acids or alcohol, but is very soluble in alkalies. The chyme and the parts of the food pressed in the third stomach are evacuated into the duodenum, and brought into contact with the alkaline secretions of the liver and pancreas. The chyme becomes an emulsion containing globules; the albumen remaining in the vegetable matter, is extracted during the passage through the intestines, whilst, by a particular set of vessels commencing on the surface of the latter, the nutritive portion called chyle is absorbed, conveyed to the thoracic duct, and from thence into the sanguiferous system. The chyle of the sheep and horse is white, opalescent, readily coagulating when received into vessels, the clot swimming in the serum, which ultimately separates: it becomes slightly reddened on exposure to air.

One ounce of chyle was obtained from a moderately large sheep: the coagulated portion washed, compressed in a cloth and dried, gave 0.424 grammes; it was more soluble in alkalies than fibrin, but composed like it of white globules, having the same general properties. The serum, gradually evaporated, weighed, when dry, 2.332 grammes; hot water dissolved 0.106 grammes of jelly from it.

As to the manner in which these phenomena were produced, it would appear that the soda contained in the fluids of the two first stomachs extracts albumen from the vegetables, and changes a portion of it into jelly: a view confirmed by the following experiment. To pure white of egg was added a solution of 2.424 grammes of soda, in 183 of water, the whole well mixed, and left exposed to air; as usual, it became a transparent yellow jelly. In 24 hours the whole had become fluid; carefully evaporated, it became brown; as it became more concentrated, some transparent insoluble films formed, and when the production of these had ceased, the whole was filtered through a cloth: further evaporation produced pellicles, which dissolved when stirred in the solution, and ultimately, when cold, the whole became a gelatinous mass, like that obtained from the second stomach.

The albumen in solution meets in the fourth stomach with free acid, which Dr. Prout has shewn to be the muriatic acid. This

acid is the second essential condition to digestion in vertebrated animals ; without it no globules of chyle would be formed. In order to ascertain the part of the stomach by which the acid was secreted, that organ taken from a rabbit was emptied, washed several times with solution of soda, and then a cloth introduced, coloured by a vegetable blue colour ; after six hours it was reddened principally near the middle region of the stomach, and several repetitions of the experiment gave the same result. Similar means indicated the same result with regard to the stomach of the sheep, &c.

The conclusions arrived at by the authors of the memoir are, that 1. The changes occasioned by digestion are purely chemical, and are not influenced by the vitality of the organs in which they take place : they may all of them, with the exception of those produced in the absorbent vessels, be imitated artificially by means of the fluids furnished by the secretory organs, namely, the soda and acid.

2. The soda is the agent to which the gastric juice owes those dissolvent properties which so much astonished Spallanzani.

3. The albuminous globules which, by their re-union form the chyme, are precipitated by the muriatic acid : this is a secretion of the fourth stomach in ruminating animals, and of the middle region of the stomach in vertebrated animals, in which this viscus is not subdivided.—*Bib. Univ.* xxvii. 229.

7. *White and Household Bread*.—Dr. Magendie tried the experiment of feeding dogs upon white bread and water, but all the animals died within 50 days, whilst those to whom he had given household bread, (pain de munition,) which only differed from the white bread by retaining a quantity of the bran, continued to thrive very well upon it. It is remarkable that one of the dogs that died, had been put upon his usual nourishment between the 40th and 45th days, but nothing could save him from the fatal effects of white bread.—*New Mon. Mag.* xv. 115.

8. *Properties of Margosa Oil*.—Mr. Allsop, in a letter from Madras, describes the oil obtained by expression from the nut or seed of the margosa-tree as having valuable medicinal properties, and acting as a preservative of perishable substances of various kinds. About $1\frac{1}{2}$ ounce of the oil is obtained from a pound of the nuts. It (and also the leaf of the tree) is applied externally for pains in the joints, swellings, stings or bites of insects, &c., and is a chief ingredient in the decoctions of the natives for flatulency, indigestion, &c. Mr. Allsop was himself relieved from a very severe attack of lumbago by three applications of the oil.

The natives besmear their *holays* or *cadjares*, on which their vedas, histories, &c., are written, with it. Some, upwards of two centuries and a half old, were nearly as fresh and in as good con-

dition as those recently taken from the tree. Mr. Allsop thinks that the oil applied to the shelves, sides, &c., of bookcases, trunks, &c., will prevent insects or vermin approaching them, and would also be found useful in preserving cables, cordage, canvass, leather, or articles of any description, which are liable to be attacked by worms or other vermin.—*Tech. Rep.* vii. 17.

9. *Quantity of Rain which falls at different heights*—For phenomena of this kind observed by M. Flaugergues, see vol. xviii. p. 186, of this Journal. There are two rain gauges at the observatory at Paris, one on the summit of the building, the other in the court yard; the difference being 27 metres, or 86 feet. The quantity of liquid collected in them is never equal, the lower vessel always collecting more than the upper. The mean of eight years gives for the rain in the lower gauge 56.136 centimetres, and for the upper gauge 49.551 centimetres, so that the difference of 86 feet has occasioned an augmentation of one-eighth. The cause of this phenomenon does not seem referable either to the direction of the wind, or to the drops gathering water from the lower stratum of air, and is as yet inexplicable. The circumstance may be important at times in estimations made of the quantity of rain fallen at the same place in different periods.—*Ann. de Chimie.* xxvii. 397.

10. *Temperature on the Earth's Surface*.—From a general and extensive review of the various experimental data respecting the temperatures observed at different places on the earth's surface, the Editor of the *Annales de Chimie* deduces the following consequences:

In no place on the earth's surface, nor at any season, will a thermometer raised two or three metres above the soil, and sheltered from all reverberation, attain the 37° of Reaumer, or 46° centigrade, (114°.8 Faht.)

On the open sea, the temperature of the air, whatever be the place or season, will never attain 25° Reaumer, or 31° centigrade, (87°.8 Faht.)

The greatest degree of cold ever observed on our globe with a thermometer suspended in the air is 40° Reaumer, or 50° centigrade below zero (— 58°. Faht.)

The temperature of the water of the ocean, in any latitude, or at any season, never rises above 24° Reaumer, or 30° centigrade, (86° Faht.)—*Ann. de Chimie,* xxvii. 432.

11. *Heights of Mont Blanc and Mont Rosa*.—Mr. de Welden, after a very elaborate examination of the various measurements of Mont Blanc and Mont Rosa, gives the following as the results, which appear to be most accurate:

Mont Blanc	2461 toises, or 15737 feet
Mont Rosa	2370 toises, 2 feet, or 15157 feet.

12. *Medicinal Application of Leeches.*—At a sitting of the Academy of Sciences, M. Dumeril reported on a memoir of MM. Pelletier and Huzard, on leeches. The authors proposed to determine, in the first place, the causes which in certain cases rendered the wounds made by leeches very difficult to heal; and in the second to ascertain the circumstances in which particular leeches will not attach themselves to the skin to which they are applied. On the first point they are of the opinion of those who attribute the difficulty to the temperament of the patient, or to the nature of the disease, or to the imprudent custom which some persons have of disturbing and tormenting the animal in all sorts of way, in order to make it loose its hold, when it has been supposed to suck too long. On the second point they have ascertained, that frequently in commerce leeches are sent to the market in every respect resembling in appearance those which are known as medicinal leeches, but which, nevertheless, are entirely different in their internal organization. The false leeches have not the mouth furnished with cutting jaws, nor can they penetrate the skin of animals; their intestinal canal and stomach are differently formed.—*Ann. de Chimie*, xxviii. 96.

ART. XV.—METEOROLOGICAL DIARY for the Months of December, 1824, and January and February, 1825, kept
at EARL SPENCER'S Seat at Althorp, in Northamptonshire.

The Thermometer hangs in a North-eastern Aspect, about five feet from the ground, and a foot from the wall.

For December, 1824.										For January, 1825.										For February, 1825.									
Thermo- meter.					Barometer.					Wind.					Thermo- meter.					Barometer.					Thermo- meter.				
Low					Morn.					Morn.					Low					Morn.					Low				
High					Eve.					Eve.					High					Eve.					High				
Wind.					Morn.					Morn.					Wind.					Morn.					Wind.				
Morn.					Eve.					Morn.					Morn.					Eve.					Morn.				
Eve.					Wind.					Eve.					Eve.					Wind.					Eve.				
Days.					Days.					Days.					Days.					Days.					Days.				
Wednesday	1	39	39	39	29.07	29.20	W	W	W	W	W	W	W	W	47	50	29.87	29.87	WbS	WbS	W	W	W	W	46	49	29.95	30.10	WNW
Thursday	2	31.5	35	35	29.40	29.32	W	W	SE	W	W	W	W	W	42	45	29.70	29.90	W	W	W	W	W	W	33	43	30.27	29.80	W
Friday	3	31	35	35	29.32	29.40	W	W	W	W	W	W	W	W	45	45	30.14	29.92	W	W	W	W	W	W	41	49	29.30	29.40	WbS
Saturday	4	33	34	34	29.44	29.38	E	NNE	W	W	W	W	W	W	45	45	29.75	30.10	NW	NNE	W	W	W	W	28	33.5	29.50	29.60	WbN
Sunday	5	33	35	35	29.43	29.78	W	W	W	W	W	W	W	W	48	48	30.38	30.43	N	N	W	W	W	W	21	35	29.61	29.60	W
Monday	6	33	35	35	29.70	29.40	W	W	W	W	W	W	W	W	48	48	30.38	30.47	N	N	W	W	W	W	27	35	29.69	30.07	NW
Tuesday	7	33	38	38	29.30	29.37	W	W	W	W	W	W	W	W	37	38	30.38	30.32	W	W	W	W	W	W	28	40	30.06	29.90	SW
Wednesday	8	33	43	43	29.65	29.57	W	W	W	W	W	W	W	W	39	39	30.55	30.62	NW	NW	W	W	W	W	32	45	30.81	29.91	W
Thursday	9	34	42	42	29.57	29.48	W	W	W	W	W	W	W	W	39	40	30.70	30.70	NW	NW	W	W	W	W	32	45	30.30	30.35	W
Friday	10	34	43	43	29.70	29.91	W	W	W	W	W	W	W	W	36	37	30.70	30.78	NW	NW	W	W	W	W	32	45	30.30	30.40	W
Saturday	11	36	46	46	29.91	29.93	W	W	W	W	W	W	W	W	34	41	30.61	30.58	WbN	WbN	W	W	W	W	31	45	30.40	30.48	WSW
Sunday	12	40.5	49	49	30.08	30.17	W	W	W	W	W	W	W	W	34	41	30.53	30.50	W	W	W	W	W	W	31	45	30.40	30.48	W
Monday	13	44	49	49	30.18	30.25	W	W	W	W	W	W	W	W	34	41	30.40	30.30	W	W	W	W	W	W	31	45	30.40	30.48	W
Tuesday	14	41	47	47	30.29	30.20	WbS	WbS	W	W	W	W	W	W	39	43	30.32	30.18	WbS	WbS	W	W	W	W	38	42	30.30	30.38	W
Wednesday	15	42	49	49	29.93	29.72	WbS	WbS	W	W	W	W	W	W	41	43	30.10	29.9	W	W	W	W	W	W	38	45	30.03	29.91	SE
Thursday	16	36.5	43	43	29.79	29.82	WbN	WbN	W	W	W	W	W	W	37	44	29.60	29.45	SSW	SSW	W	W	W	W	38	45	29.87	29.85	SE
Friday	17	21	43	43	29.86	29.90	WbS	WbS	W	W	W	W	W	W	35	40	29.63	29.80	WbS	WbS	W	W	W	W	32	47	29.85	29.78	SW
Saturday	18	21	40	40	29.98	29.65	W	W	W	W	W	W	W	W	35	47	29.20	29.20	SSW	SSW	W	W	W	W	42	44	29.80	29.57	SW
Sunday	19	45	53	53	29.92	29.83	W	W	W	W	W	W	W	W	35	47	29.28	29.30	W	W	W	W	W	W	41	47	30.08	30.08	SE
Monday	20	46.5	50	50	29.91	29.83	W	W	W	W	W	W	W	W	35	42	29.28	29.30	W	W	W	W	W	W	39	49	30.10	30.13	SE
Tuesday	21	34	50	50	29.27	29.50	SW	SW	W	W	W	W	W	W	36	42	29.70	29.74	N	N	W	W	W	W	29	46	30.30	30.30	NW
Wednesday	22	30	35	35	29.88	29.80	WSW	WSW	W	W	W	W	W	W	41	41	29.80	29.60	NE	NE	W	W	W	W	28	45	30.31	30.21	WbN
Thursday	23	30	35	35	29.60	29.80	SW	SW	W	W	W	W	W	W	31	38	30.10	29.80	NE	NE	W	W	W	W	37	40	30.10	30.08	SE
Friday	24	30	40	40	29.30	29.27	SW	SW	W	W	W	W	W	W	31	39	30.00	29.80	W	W	W	W	W	W	35	40	30.10	30.17	SE
Saturday	25	30	43	43	29.63	29.93	W	W	W	W	W	W	W	W	35	39	29.52	29.78	W	W	W	W	W	W	29	39	30.27	30.36	SE
Sunday	26	35	53	53	29.70	29.70	WbS	WbS	W	W	W	W	W	W	38	50	29.88	29.81	S	S	W	W	W	W	29	39	30.14	30.06	NE
Monday	27	45	47	47	29.93	30.08	W	W	W	W	W	W	W	W	34	43	30.57	29.81	SW	SW	W	W	W	W	32	37	30.14	30.06	SE
Tuesday	28	31.5	37	37	29.93	30.10	W	W	W	W	W	W	W	W	34	43	30.57	29.81	SW	SW	W	W	W	W	32	37	30.14	30.06	NE
Wednesday	29	31	37	37	29.91	30.10	W	W	W	W	W	W	W	W	34	43	30.57	29.81	SW	SW	W	W	W	W	32	37	30.14	30.06	SE
Thursday	30	40	51	51	29.93	29.93	WbS	WbS	W	W	W	W	W	W	37	50	30.36	30.36	W	W	W	W	W	W	33	43	29.63	29.46	W
Friday	31	40	51	51	29.93	29.93	WbS	WbS	W	W	W	W	W	W	37	50	30.36	30.36	W	W	W	W	W	W	33	43	29.63	29.46	W

